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CONTENTS

'The Place of Theory in Parapsychology'[1])

By John Beloff

The American philosopher of science, Michael Scriven, once remarked that, whereas psychoanalysis was all theory without any hard facts, parapsychology was just a collection of facts without any theory[1]). I am not here concerned with the accuracy of the first half of his statement but, so far as the second half is concerned, it would be hard to deny that, though there has never been any lack of theorizing in this field, there is nothing that we could call an agreed theory. Indeed, one reason why most scientists are still content to ignore the findings of parapsychology is, not just that the facts themselves are contro-versial, but that they remain obdurately outside any known theoretical framework. Nor is this just some accidental defect in parapsychology, the point is that the facts would simply not qualify as 'paranormal' in the first instance unless they were anomalous in just this way. Worse still, from the standpoint of the conventional scientist, where they cannot just be dismissed as so many meaningless anomalies they appear to be a hang-over from some pre-scientific tradition of magic or occult lore which makes them, not merely alien to the scientific outlook, but positively subversive of all sane and rational values.

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1) This article was presented by Dr. John Beloff at a seminar, held at the Psychological Laboratory of the University of Utrecht, on April 14, 1972. Dr.Beloff is a lecturer at the Department of Psychology, University of Edinburgh. He is also a noted parapsychologist and serves this year as the president of Parapsychology Association.

It is not surprising, therefore, that those scientists who, rightly or wrongly, have become convinced of the reality of paranormal phenomena have been at pains to show that the facts can be made intelligible, that the term 'paranormal' has no more than a provisional connotation and that, in the wider perspective of tomorrow, these so-called paranormal facts will be seen to possess a rationality or coherence of their own. It is this desire to meet the critics on their own ground that has given a special urgently to the question of the place of theory in parapsychology.

However, before we can say anything useful on this question, we must first say something about the place of theory in science generally. There are at least two reasons for wanting a theory in any given field of science. In the first place, without one, discoveries or inventions would have to depend on some purely piecemeal procedure of trial and error and sheer good luck and progress would inevitably be slow. Some progress could no doubt still be made - we tend to forget that before the late 19th century technological advances depended hardly at all on theoretical science - but, undoubtedly, there could be no systematic research as we now conceive of it in the absence of theoretical understanding. But, in the second place, setting aside all such pragmatic considerations, it could be argued that theory is the ultimate aim of science, that science exists for the sake of its theories rather than the theories for the sake of science, that facts are of no interest except insofar as they illustrate some theory, that, finally, a theory is an achievement which, like a work of art, must be appreciated for its own sake and not for any practical consequences that may flow from it.

But, whatever view you take of the role of theory in science, parapsychology is a sad example of what happens where theory is lacking. Progress had indeed been slight - experimental parapsychology, after all, is as old as experi-

mental psychology -, research workers at a loss to know what to do next have nothing to fall back on except to repeat whatever has been successful in the past and such discoveries as they have nearly always been due to good fortune rather than to intelligent forethought. On the other hand, it would not be quite true to say, in this field at least, that facts are of no interest without a theory. Here, I suggest, we must be careful to draw a distinction between the natural sciences and the human sciences. In nature, it may well be true that facts have little intrinsic interest apart from some theoretical superstructure they are taken to support; in human affairs, on the other hand, facts may interest us simply because we are ourselves human. Thus, there is little that one could call theoretical about human history and yet most of us consider that history is worth knowing. Parapsychology might be regarded as, in the first instance, a contribution to the natural history of man. For it is, surely, intrinsically interesting and important to discover the limits of human capacities in whatever direction and, in addition, there is a certain element of the marvellous and the astonishing which attaches to anything of a paranormal nature. Obviously, if parapsychology were to yield some fundamental new insight into the workings of the mind or of the universe at large this would be the supreme bonus but, even were we to do no more than establish beyond all doubt and cavil simply that ESP exists, this, in itself, would be a striking fact about the world quite apart from any further interpretation we were to place on it.

The word theory has never been quite free of ambiguity so, for what is to follow, I would like to distinguish between a stronger and a weaker sense of that term. For any doctrine to qualify as a theory in the strong sense it must be possible for us to derive predictions from it which could not have been anticipated in the absence of that theory. In the advanced sciences, new entities are sometimes predicted on purely theoretical

grounds before there is any empirical evidence to support them. Perhaps the most famous example in the whole history of science concerns the discovery of the planet Neptune. A problem arose because the behaviour of the planet Uranus appeared to be at variance with Newtonian theory. In the year 1846, Adams at Cambridge and Le Verrier at Paris both independently suggested that the theory could be saved if we postulated another, as yet unobserved planet, having certain specified properties. Such a planet was soon after located and identified by the astronomer Galle of Berlin and was named Neptune. Many similar instances could be cited from the annals of modern nuclear physics, for example Yukawa's prediction of the meson or Pauli's prediction of the neutrino before ther was as yet any experimental evidence for the existence of these fundamental particles. But I mention such cases only to dismiss them because I do not think they have any relevance even to the biological, let alone the psychological sciences. For example, our knowledge of the brain has been developing very rapidly over the past few decades but I am not aware of any brain mechanism which was discovered purely as a result of theoretical considerations. Such discoveries were usually a by-product of using the techniques of ablation and direct stimulation in an empirical, exploratory way. For example, the remarkable discovery by James Olds in 1953 of pleasure-centres in the rat's brain arose out of an accidental misplacement of an electrode! In psychology, the fate of Hullian theory stands as a warning of what is liable to happen if one attempts to impose on the evidence a theory that is too strong for it to carry.

Strong theories, in short, are a feature of advanced, exact sciences; elsewhere we are forced to rely on a waker type of theory or, as it is more usually referred to these days, a 'theoretical model'. A model serves to guide us in framing our hypotheses but they are not strict deductions from it and if they are falsified we

are not obliged to scrap the model, we can merely tinker with it and readjust it. A model will usually be retained as long as it supplies a framework into which we can fit our observations and by reference to which we can conceptualize our knowledge. Sometimes a model may be little more than an extended analogy. An example of one such model from 18th century science was the so-called 'caloric theory' of heat. This was based on treating heat as a special kind of fluid and, although eventually it had to be discarded, it served in the meanwhile to coordinate a variety of observations, for example on the flow of heat from one body to another. Similarly with the aether of 19th century physics until it became redundant with the advent of relativity theory, this was based on treating electromagnetic radiation on an anlogy with sound-waves. Ever since its inception as an experimental science, psychology has toyed with innumerable different models of the mind or of this or that mental function, most of them drawn from engineering or from one of the better understood sciences. At the present time, the favourite models, at least in the cognitive sphere, are those borrowed from computer science. Indeed, ever since we have been able to construct artefacts that exhibit mind-like behaviour the temptation to assume that the human mind functions according to similar principles has proved hard to resist. Many of the current models of memory, attention, perception and so on have reached high levels of sophistication. They consist, for the most part, of flow-diagrams indicating the sequence in which information might be processed or analysed in various ways so that they represent, in effect, a possible program according to which the brain is supposed to operate.

If parapsychology is ever to achieve the status of a theoretical science, it too, no doubt, will make use of models taken from other sources.[2] The situation, however, is radically different from that which holds for the rest of psychology,

in one respect at least, namely that parapsychology directly impinges on physics, or at any rate on the implied claim that every physical law has universal application. The initial mystery with a phenomenon such as ESP is how the information gets itself transferred from the target to the subject. This, on the face of it, is a physical, not a psychological question. It is this that people have in mind when they ask, usually in a somewhat simple-minded way, 'how does ESP work?'. The answer which people are clearly seeking at this stage is in terms of some special kind of a carrier capable of transmitting the relevant information. And many attempts have, in fact, been made, some of them by physicists whose interest in parapsychology has been aroused, to supply an answer to this question. How successful have they been?

Almost from the beginning the analogy of radio-communication suggested itself as the most plausible answer to the problem posed by telepathy. Sometines this was even given explicit recognition in the use of such expressions as 'mental radio'. Eventually, however, it began to look less and less likely that electromagnetic radiation could be the basis of telepathy and so this first model fell into disrepute. Without going into too many details I may mention at least three considerations that appeared to tell against the electromagnetic hypothesis: (a) the electrical activity of the nervous system is so feeble that it is barely detectable more than a very short distance from the surface of the body and the brain would therefore be useless as a transmitter; (b) there is little evidence to suggest any attenuation of the effect with distance, on the contrary, in the 1940s a successful series of ESP tests was carried out with the experimenters at Duke University, North Carolina and the subject in Yugoslavia and there have been other successful transcontinental experiments since then[3]; (c) putting the subject into a Faraday Cage, a device designed to eliminate most of the electromagnetic spectrum, fails to inhibit

ESP. This observation was first reported by Vasiliev in Leningrad in the late 1930s[4] and has recently been corroborated by Puharich in the United States who even claims that this procedure enhances the effect![5] With the exception of one solitary Russian investigator who still maintains that telepathy works by means of very long wavelengths, of the order of 10 km.,[6] I know of no one who still takes seriously the electromagnetic theory.

But, even apart from these particular considerations, there was a deeper reason why a more sophisticated model was deemed necessary. All the experimental evidence pointed to the fact that the differences between the different varieties of ESP, i.e. between telepathy, clairvoyance and precognition, were of a superficial kind which had more to do with the design of the experiment than with the actual nature of the phenomenon. For example, the same conditions which appear to favour one appear equally to favour the others and, though there are exceptions, the same subject who excels at one usually excels at the others.
Consequently a model which was relevant to one but which left the others as mysterious as before would look too much like special pleading. Besides, if any of the three varieties of ESP is to be regarded as more fundamental than the others, it should be clairvoyance since this does not require the participation of another person to act as agent or 'sender'. But for clairvoyance, a model more akin to radar than to radio would seem to be required since we can scarcely suppose that a written symbol inscribed on a piece of paper can act as a transmitting source.

Undaunted by the heroic dimensions of their task the theorists responded to the challenge with fresh flights of ingenuity. Some of the remoter and more paradoxical implications of relativity and quantum theory were brought to bear upon the problem and, latterly, the literature has sprouted a veritable plethora of strange new entities which includes faster-than-

light particles or 'tachyons',[7] particles with negative energy or methematically imaginary mass or 'psitrons', time-reversal effects, extra dimensions of time and/or space and, of course, that standby of the 'paraphysicists', new types of field forces or 'psi fields'. Those who enjoy such speculations should consult those who enjoy expounding them. There is, for example, an excellent article by the late Adrian Dobbs, himself a physicist, entitled 'The Feasibility of a Physical Theory of ESP'.[8] Dobbs' own theory of precognition is among the most sophisticated of its kind.[9] In fairness I should warn you, however, that, unless you are much more at home than I am with current ideas in fundamental physics, it is doubtful whether any of these theories will make much sense to you. I am certainly not competent to pronounce on them and must defer therefore to those who are but they have, I may say, been severely criticized on their own terms of reference by Chari,[10] a polymathic Indian philosopher and the only man I know who seems equally conversant with the literature of quantum theory, of physiology and of parapsychology!

There are, however, certain general comments which I can make that would apply to all attempts so far to extend the laws of physics so as to embrace those phenomena which we now call paranormal. First, none of them, to the best of my knowledge, has got beyond the initial question of transmission. Yet it is obvious that before we can be said to have even the beginnings of an explanation for some given communication-system, be it radio, television or the perceptual process in animals, we have got to be able to say, if only in general terms, how the information is encoded at the source, how it is decoded at the receiver and, more especially, how the message is finally extracted from among the totality of background information. Now, the irony of the situation is this: the better these theories are able to explain how the information might be transmitted from target to subject, the harder it becomes for them to explain how the information is decoded. The point is that if you are going to postulate a form of energy-transfer

which does not decrease with distance, which can penetrate any material barrier and which can, if need be, jump across intervals of time you have an exceedingly difficult problem on your hands of suggesting an appropriate filtering or focussing mechanism which the subject could use to eliminate all energy except that which amanates from the target. Secondly, none of these theories, and again I speak subject to correction, has yet yielded a precise testable prediction. They remain free-ranging speculations with no anchorage in experimental data and, not surprisingly, they have had no influence on the actual practice of parapsychological research. They are, in fact, more in the nature of an intellectual game or exercise than of a theory in the accepted sense. And, however much fun the game may be for the players, for the on-lookers they are liable to appear somewhat inconsequential.

It is not my purpose, however, to knock such theories or to condemn them as valueless. But their value, as I see it, is roughly on par with that of science-fiction. By this I mean noting derogatory, rather that both activities serve to stretch the scientific imagination and, by doing so, help to prevent any premature dogmatism about what is or is not possible in the actual world. Commonsense, let us not forget, is merely a disguised form of dogmatism and those sceptics who like to pride themselves on a robust commonsense often betray merely a lack of imagination. There is also another, perhaps more cynical reason why I would not want to discourage such theorizing: it helps to sustain an interest in parapsychology among physicists. On the whole, physicists have shown themselves much more hospitable to parapsychological ideas than have psychologists. Yet, anxious as I am to interest as many scientists as possible in parapsychology I am, as no doubt you will have gathered by now, basically sceptical of a solution being found along conventional scientific lines. The phenomena strike me as being just that much too wild, too

improbable, too far-out, to lend themselves to the kind of approach that has served us so well with respect to the rest of nature during the past four centuries.

Let us look next, therefore, at the consequences of adopting a radically different approach. Let us see what sort of theories would be open to us if we were to drop the assumption that reality is all of one piece and start from the assumption that, with respect to mental phenomena at least, laws of a fundamentally different kind might prevail. We might perhaps call theories of this type paranormal theories as opposed simply to theories of the paranormal. An example which has lately received some publicity, as a result of having been resuscitated by Arthur Koestler, starts from the assumption that over and above the causal laws on which science has mainly concentrated there exist certain a-causal laws and that it is a law of this latter kind which accounts for the events we now regard as paranormal.[11] The idea seems to have originated with that unorthodox Austrian biologist, Paul Kammerer, to whom Koestler devoted his previous book[12]. Kammerer was an inveterate collector of coincidences and, on the strength of these, he came to the conclusion that similar but otherwise independent events cannot occur as we have always assumed at random, that is according to the laws of probability, but tend instead to cluster together in space and time as if attracted to one another simply by virtue of their inherent similarity. He called this the 'Law of Seriality' and published a monograph with this title in 1919.

The next thinker to take up this idea was no less a person than C.G. Jung who, in 1952, published a short treatise entitled: 'Synchronicity: An A-Causal Connecting Principle'[13]. Jung opens his exposition somewhat ungraciously by first dismissing all of Kammerer's precious coincidences as easily explicable by chance alone. He then offers the reader an assortment of truly staggering coincidences which, though they are mostly retailed at second or third hand, like the inimitable tale of M. de Fontgibou and the plum

pudding, he clearly expects the reader to take seriously. Jung does, however, go one better than Kammerer in this respect: he carries out an actual experiment on coincidences. For this he collected data on a large batch of married couples by having their horoscopes analysed for certain astrogological correspondences, those in fact traditionally associated with marriage, and compared the results with those obtained from randomly matched pairs. And, lo and behold, he got a highly significant result! Now, whatever you may think of this experiment, it is only by considering some preselected sample of this kind that we could hope to throw any light on Kammerer's original question, namely: do coincidences occur more often than they should? So long as we stick to the spontaneous coincidences of real-life we can never answer this question if only because probabilities cannot be assessed retrospectively.

Unlike Kammerer, who was a sceptic on this score, Jung was, of course, a convinced believer in the paranormal and, in his autobiography gives many striking instances of it in his own life. He suggested, however, that it was useless to try and explain either ESP or PK in causal terms, i.e. as a species of mind-matter interactions, since this would amount to a magical type of causation for which there was no place in the scientific outlook. His concept of 'synchronicity', on the other hand, allowed one to treat any observed correspondences between target and response as, at one and the same time, coincidental and yet meaningful! Jung then gave his concept a further dynamic twist, which linked it up with his own clinical observations, by suggesting that such meaningful coincidences would for some reason be more likely to occur when the atmosphere was emotionally charged.
This stipulation further helped to explain why parapsychological findings so rarely stand up to repetition: the original excitement, which brought them into being as it were, has evaporated.

Just as Jung begins by slighting Kammerer, so Koestler introduces his version of the theory by criticizing Jung whom he finds confused and inconsistent. It is not so easy, however, to discover just where Koestler's theory differs from that of Jung. What does emerge is that Koestler's concept of 'confluent events' which is supposed to supersede both Kammerer's 'seriality' and Jung's synchronicity' has a still wider compass. Koestler hopes that it will throw some light not only on parapsychological phenomena but also on such questions as the inheritance of acquired characteristics which, following Kammerer, he still takes seriously or the hierarchical structure of living things which he has discussed at length in previous volumes. Here, however, we shall be concerned only with its implications for parapsychology.

What, then, are we to make of this puzzling, not to say paradoxical notion? Undoubtedly it offers one escape route from the demands for a causal mechanism which, as we have seen, is likely to lead only to futility. As a theory of the paranormal, however, it would have more to recommend it if all the evidence we had was of the spontaneous variety. If, for example, the night my grandmother dies her portrait accidentally falls off the wall we should be sorely tempted to call that a meaningful coincidence. It is less easy, however, to make the idea sound plausible when we are dealing with the experimental evidence. An experiment is, by definition, a situation where a certain independent variable is deliberately manipulated in order to produce a certain change in some dependent variable. If the experiment is a success, that is if we can demonstrate that this effect is statistically significant and not due to some uncontrolled artefact, then we have surely all we need to constitute a causal relationship whatever the variables in question. If, for example, I could make it rain whenever I recited a certain ritual incantation, I would be justified in claiming that I had causal control over the rain-

fall even though I knew noting more about the nature of this control. In other words, there is nothing in the logic of cause and effect which would limit the concept to that of mechanical causation. To treat parapsychological effects, as Koestler would have us do, as exemplifying 'confluent' rather than 'causal' relationships is to treat them as a species of divination. All divinatory practices, from the auguries of the Romans to the 'I Ching' (or book of changes) of the Chinese are predicated on the assumption that a meaningful coincidence can arise between a fortuitous oracle and some future event on which we seek guidance. As a model, however, I cannot see that it has much to offer parapsychological research.

Let us therefore see what follows if, in defiance of Jung or Koestler, we boldly posit a non-mechanical causation as the basis of psi phenomena. Suppose we attribute to mind certain transcendental powers that would allow it some measure of independence from the physical parameters which we take as defining the real world. Suppose, in particular, we start with the admittedly colossal assumption that any item of information that can be identified is potentially, accessible to us no matter where in the universe the target is situated relative to our bodies, whether in time or space, and no matter how that information is encoded. If, for the sake of argument, we start from that end and take as axiomatic what otherwise would seem most in need of explanation, the traditional position is reversed and what we then have to ask is: why, if that be the case, are we hardly ever able to acquire any information at all other than that which passes through the normal sensory channels? The idea that this might be the right question to ask in the circumstances stems from the French philosopher, Henri Bergson, who, in 1896, wrote a book entitled La Matière et La Mémoire [14]. At that time Bergson was not concerned with anything of a paranormal nature,

he was interested in propounding a metaphysic which would escape the worst perplexities alike of Realism and Idealism. Thus, against traditional Realism he could never bring himself to believe that brain processes could of their own accord generate percepts. He therefore adapted instead a position more akin to naive realism according to which the percept is not just a subjective representation of external reality but an actual slice of that reality. Again and again, in his book, Bergson keeps insisting that the brain is not concerned with knowledge as such, it is mind or memory or spirit, call it what you will, that is the organ of knowledge, the brain and nervous system is concerned purely with behaviour; with action. The elaborate sensory and perceptual apparatus of the organism cannot therefore be that which gives us our knowledge of the world, rather it is that which restricts our knowledge and confines it to that small segment of the environment which is of immediate importance to us as separate biological entities. In brief, the brain is primarily a filter which evolution has devised to insure our attention to the here and now, "c'est l'organe de l'attention à la vie" in Bergson's famous phrase. In more recent times this bold idea has been favourably considered by two, eminent English philosophers, H.H.Price of Oxford and the late C.D. Broad of Cambridge[15], and an attempt to explore in detail its implications for perceptual psychology has been made by M.M. Moncrieff in a book entitled The Clairvoyant Theory of Perception.[16] I myself believe that, far-fetched as it must seem, it is still the most hopeful point of departure we have for any general theory of the paranormal.

No filter is ever perfect and so we could regard parapsychology as being concerned with those special cases where, owing to some innate defect or leak in the filtering system, information reaches the mind direct form the outside world instead of indirectly via the senses. Looked at in this way many puzzling

features of the evidence at once become more comprehensible. There is, first of all, the fact that, since ancient times paranormal awareness has been associated with altered states of consciousness; with trance-states, with ecstasy, with intoxication and frenzy, with deep meditation or the mind-expanding routines of mysticism, with almost anything that enables to elude the trammels of the normal waking state. Even the dream state has traditionally been regarded as a potential outlet of this kind and it is worth noting that one of the most interesting developments of recent experimental parapsychology, the work of the Maimonides group in New York, is based on using dreams as a means of demonstrating ESP, exploiting for the purpose the new techniques of modern sleep research.[17] The filter-theory may also help to explain what so many investigators have discovered to their cost, namely the evanscence of psi phenomena. In place of the learning-curves that we find with all other skills, psi ability is characterised by decline-effects. Now, decline-effects are just what we look for in the case of habituation phenomena where an organism reacts to rid itself on some disturbing stimulus, In our present analogy we must suppose that if, by some mishap, the filter is penetrated, it rallies to repair its defences, becomes more efficient with the result that ESP is thereafter excluded. Then there is the curious phenomenon known to parapsychologists as 'psi-missing' which consists in consistently negative scoring. It is directly analogous to the phenomenon of perceptual defence in experimental psychology and suggests likewise the existence of a second line of defence whereby information which does manage to get through will not be recognized or acted upon.

The mention of 'perceptual defence' brings me to the last section of this paper. Recently, when I read Norman Dixon's new book on Subliminal Perception[18] it struck me forcibly that so many of the properties which he attributes

to subliminal perception (or SP as we may call it) could equally be attributed to ESP. The gist of Dixon's theory is that when a stimulus falls below a certain critical level of intensity it tends to elicit a response which is not just weaker, but qualitatively different from the response which it elicits at normal intensities. In other words true Dixonian SP involves not merely a certain amount of discrimination in the absence of awareness but a different kind of processing mediated by a different set of brain mechanisms capable of operating in parallel with those which mediate ordinary conscious perception. For example, if the stimulus is a word and the subject, though not consciously aware of seeing anything at all, is asked to guess what the word is, he will respond with a word which is not necessarily similar structurally to the stimulus word but is related to it semantically. Very often, especially if the stimulus word has some emotional connotation for him the transformation which occurs will be of a symbolic kind and may have decidedly Freudian overtones.

One of the more bizarre examples of this sort of thing which Dixon discusses in his book at some length is the so-called 'Poetzl Phenomenon' after the Austrian neurologist O. Pötzl who first demonstrated it in 1917. What Pötzl did was to present his subjects with a rather elaborate picture which he exposed once, for a 1 m.s. interval, in a tachistoscope. If asked to do so immediately following this exposure the subject was quite unable to reproduce more than the barest fragment of the picture. However, if asked the following day to describe his dreams of the preceding night a greater than chance resemblance could be discerned between features of his dreams and features of the stimulus picture. Recent work on the Poetzl phenomenon, using, I may say a rigorous experimental design, has shown that the same effect can be obtained without resorting to dreams if, following the exposure, the subject is merely asked to relax and either to free-associate or to describe any images or fantasies

which spontaneously emerge into consciousness. Dixon attempts to explain such findings by suggesting that SP depends, not on the classical arousal system which sustains the cortex in a state of vigilance and involves the R.A.S., but on a secondary arousal system which comes into play during the dream state or when vigilance is relaxed and involves the limbic system. Its biological function, as Dixon sees it, is to allow the organism access to a much wider range of stimulation than is possible with the selective attention which typifies supraliminal perception. SP is, of course, a most unreliable form of perception, nevertheless it does, he believes, contribute something towards the ultimate efficiency of performance.

How far dare we press the analogy between SP and ESP? For a start we must distinguish between ESP as manifested under two different conditions, that of the forced-choice test and that of the free-response test. In the forced-choice situation a guess can be only either right or wrong, there is no question of it being qualitatively similar or dissimilar to the target symbol. In this situation a good subject (and I must remind you that there is nothing in the world quite as rare as a good subject) can, hopefully, score at a statistically significant level above chance. We then say that our subject is using his ESP though we must remember that he has no conscious perceptual experience of the target and no clue as to whether he is right or wrong on any particular trial; his success is at the purely behavioural level of guessing. A comparable effect can be obtained if a forced-choice situation is used with subliminal stimuli. Indeed, from the subject's point of view he might just as well be doing an ESP test since he is equally unaware of the target though it should not be so difficult under these conditions to demonstrate a statistically positive effect.

With a free-response test, however, we find something much closer to Dixonian SP or

to a Poetzl effect. Suppose, for example, the task you set your subject is to reproduce a message or a picture presented inside a sealed-opaque envelope or to say something relevant about the owner of some token or proxy object. Assuming that you can get results at all on this kind of a test they are likely to incorporate all kinds of distortions and disguises and the subject is likely to make plentiful use of images which spontaneously spring into consciousness. Such qualitative changes are of course specially in evidence where the protocols are based on dream experiences, as in the Maimonides experiments. Indeed, before we can demonstrate a correspondence at all in a free-response test it is usually necessary to resort to a blind matching of targets and protocols carried out by independent judges. There have in the past, I may say, been subjects so outstandingly gifted that they have succeeded in reproducing with almost perfect fidelity some complicated drawing or inscription presented inside a sealed envelope - the great Polish clairvoyant, Stefan Ossowiecki could ocaasionally acieve this feat - but needless to say it is very rare and seems to demand an almost superhuman effort of concentration.

There is thus, I suggest, good grounds for thinking that, at least so far as the psychodynamics of the process is concerned ESP is similar to SP, we might almost describe it in this context as perception at zero intensity! The work of Martin Johnson demonstrating a connection between patterns of ESP scoring and his 'defence mechanism test' which utilizes, if not subliminal at least pre-conscious perceptions, further encourages us to pursue this comparison.[19] Moreover, Dixon's conjecture as to the function of SP, namely that it enlarges the scope of perception though at a cost in accuracy and dependability, seems to apply with even greater force to ESP. As with SP, so with ESP, success is achieved, not by a deliberate effort of will but by relaxing and then concentrating on whatever impressions supervene.

It is important, however, to realize also the limitations of this model. It is useful

if we want to understand what transpires once the critical information becomes part of an individual mind, but it tells us nothing about how that mind establishes contact with the target in the first instance. In the case of SP there is no great mystery on this point since it is generally conceded that the threshold for nervous excitation will be very much lower for a given stimulus than the threshold for conscious recognition, though it is noteworthy that SP evoked considerable scepticism and resistance when it was first mooted. But, once we can no longer appeal even to a minimal stimulation, the situation is quite different. The Bergsonian philosophy of mind made ESP effectively omniscient and then appealed to our physiological nature to explain why that omniscience could not be freely exercised. But it could say little about what we might call the supreme mystery of ESP, namely how it is that we can focus on just that particular target which has been designated in the experimental instructions. It is not enough here to insist that ESP has unlimited scope, the problem is how it can also be selective and discriminative. Confronted with this tremendous problem I can sympathise with those sceptics who prefer for the time being to deny that there are any genuine cases that require explanation.

FOOTNOTES:

1. See his 'Psychoanalysis and Parapsychology' in R.G. Colodny (ed.) Frontiers of Science and Philosophy (University of Pittsburgh Press 1962)

2. Cf. C.T. Tart 'Models for the Explanation of Extrasensory Perception' Internat.J. Neuropsychiatry 2, 1966, 488-504

3. See J.B. Rhine & Betty Humphrey 'A Transoceanic ESP Experiment' J.Parapsych. 6, 1942, 52-74 also Elizabeth McMahan & J.B.

Rhine 'A Second Zagreb-Durham ESP Experiment' J.Parapsych. 11, 1947, 244-253. For later work see K.Osis 'ESP over Distance: A Survey of Experiments published in English' J.Amer. S.P.R. 59, 1965, 22-42. Actually, Osis, by simply pooling positive and negative scores shows that there is a drop in the overall averages with increasing distances! However his very fragmentary data hardly justify his speaking of a law of attenuation especially as his own findings with maximal antipodean distances are among the most significant!

4. See L.L. Vasiliev Experiments in Mental Suggestion (ISMI Church Crookham, Hants. 1963 transl. from the Russian Edition, Leningrad University 1962).

5. See H.K. Puharich 'Electrical Field Reinforcement of ESP' Internat.J. Neuropsychiatry 2, 1966, 474-486

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A NEW TECHNIQUE OF TESTING ESP IN A REAL-LIFE, HIGH-MOTIVATIONAL CONTEXT

A Written Academic Exam as a Disguised Test of Clairvoyance.

Three Studies. A Preliminary Report

by Martin Johnson

INTRODUCTION

It is often assumed that one of the limitations of testing ESP in a laboratory setting is that the situation is so quite different from that when spontaneous phenomena occur. The situation in the laboratory is said to differ unfavourable in several respects; the tests are "minimized" situations, the outcome of which has very little implications for the subject. The problem of finding psi-application in real-life is not new (1.2.3).
The situation is alien. When ESP occurs, in real-life a person is not consciously told to turn on his psi-capacity, something that he is supposed to do in the laboratory. The intrinsic motivation is very uncertain in many laboratory situations. In this article the author is going to present a new technique of testing ESP, by the use of which, the possible contribution of a person's ESP-ability in a compelling real-life situation, quantitatively can be measured. The method offers several new and in some respects advantageous features; for one thing the subjects are unaware that they take part in an ESP-test; for another their responses are not influenced by already existing response preference patterns or by the development of such patterns, during the testing procedure. This is something of potential impact under ordinary ESP-testing conditions, when the technique is based on a forced-choice procedure. There are good reasons to think that such an

effect caused by psychologically founded non-random behavior could very often exert a canceling-out influence on an operating, "weak" psi-process.

The test is composed of two well-defined parts: a cognitive one - the academic exam, of which the subject is aware and towards which he is directing his attention - and an ESP-part of which the subject is not cognitively aware. The targets of the ESP-part of the test are composed of typed information suggesting answers to the questions of the exam. If the subject could utilize the information offered by those targets through a clairvoyant process the quality of his answers to the exam could improve.

METHODOLOGY

1. A written exam, including eight questions to be answered by graduate students who had taken part in a course of interviewing, was worked out. The questions were typed on two sheets of paper; questions No. 1, 2, 3 and 4 on page 1 and questions 5, 6, 7 and 8 on page 2. The questions were related to some issues and concepts presented and discussed during the lectures delivered by the present author, responsible for the course. (Most of the questions were related to the book: "The Dynamics of Interviewing, by R.L. Kahn and Ch.F. Cannell").

Most of the questions in the exam were possible to phrase in two typed lines, whereas the spaces intended assecible for the answers on the average were 3 times 15 centimeters.

2. In each of the three studies a manual for assignment of marks to each of the eight questions worked out, to try to ascertain reliable scorings of the answers. In the first study the manual was not very detailed. In the first two studies the range of the scale was between 0 and 3 (0.0, 0.5, 1.0, 1.5, 2.0, 2.5, and 3.0); in Study III the range was between

0 and 2, e.g. a five-point scale.

3. For each of the students a test-set was prepared. Such a test-set consists of:

a) an opaque envelope.
b) two mimeographed sheets of paper with the questions of the exam - questions 1,2,3,4 on one sheet, and questions 5,6,7 and 8 on the other. The sheet with questions 1,2,3 and 4 are glued onto the front cover of the envelope; questions 5,6,7 and 8 onto the back cover of the envelope.
c) a piece of cardboard, slightly smaller than the envelope specially prepared Xerox copies of the mimeographed sheets, (the front- and the back covers).
d) a piece of aluminum foil in which the piece of cardboard, after necessary preparations can be wrapped, before being inserted into the envelope (See under 4!)

4. According to the design of the experiment, every subject functions as his own "control", which means that fifty per cent of the questions of the test-set - or four out of eight questions in a test-set-have been associated with targets attainable to a clairvoyant process within the subject. For each of the test-sets the questions that should be associated with a target were determined by a random procedure (the use of random digit numbers and by use of dice-throwing by which the entry-points in the random digit number booklet were determined). This procedure was carried out by an experimenter-assistant. It should be observed that the locations of the primary targets within the test-set were assigned i n d i v i d u a l l y for each of the potential subjects. Based on the list of assigned targets, mimeographed strips of primary targets as well as secondary targets were glued onto their appropriate places within the mimeographed copies of the exam. The next step was to reproduce these target-sheets by the Xerox-method.

This was done to secure a uniform surface-quality [1]) for all the locations of answers within the test-set. These target-sheets were later glued onto the pieces of cardboard.
The pieces of cardboard were always inserted in such a way that a certain question on the cover of an envelope, always corresponded to the same "question" (space for the answer) on the piece of cardboard, located under the cover.

5. Methodological changes and improvements in Study III.

Since Study I and to some extent Study II can be considered as pilot-studies and since the question of the reliability of the authors ratings were still uncertain after Study II, it was decided that in Study III, the hypothesis about an influence from the targets on a psi-basis, should be put to a much more severe test. More emphasis was laid on the control of the experiment and of obtaining a measurement of the degree of "intersubjectivity" of the ratings which means that the contribution of a co-rater was secured in advance of the experiment. As regards the control of the experiment, the independently made ratings, (by the experimenter at Utrecht and by the co-rater at Lund, Sweden), were sent to Dr. P.A. Vroon, Psychological Laboratory, University of Utrecht, before the test-sets were opened.
Dr. Vroon was also responsible for opening the test-sets and identifying targets and non-targets within each of the test-sets. Furthermore a change was made as regards the character

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1) One could reason that a subject's discrimination of such inequalities of the surface of the covers, by means of sensory perception, could under certain conditions have lead to a differential scoring that might not be attributable to ESP but based on information affecting the subject's curiosity and secondarily his motivation.

of the target. As is often stresses by philosophers of science, the strenght of the confirmation of a hypothesis is not only a question of the quantity of favourable cases supporting a certain hypothesis, but also to a large extent a question of the range and variation of the data confirming the theory (4).

6. In Study III, two classes of targets were used: Primary targets and secondary targets. "Primary targets" denote those targets which function as carriers of clairvoyant information relevant to the answering of the questions of the exam. By "secondary targets" are meant typed information in the places for answers on the mimeographed sheets glued onto the pieces of cardboard. This typed information is not relevant to the answering of the questions, but should, according to the rationale of the experiment, exert an influence on the subject's attitude or confidence in himself as regards his ability to answer the questions and pass the exam.
In this Study, the primary targets were carriers of "negative", that is misleading information as regards how to give correct answers to the questions of the exam. The questions had been selected and worded in such a way that it should facilitate confusion or a mistake. The hypothesized "negative" influence from the primary targets, based on clairvoyance was hopefully to be reinforced by the secondary target. When a secondary target was associated with a primary target, it expressed degrading or pessimistic assumptions about the subject's general ability or ability to give an appropriate answer and pass the exam (Example: "You never will be able to give a correct answer to this question"; "you are too stupid to pass this exam", etc.). When a secondary target was associated with the location within the exam of a non-primary target (= no information given as regards the answer to the question), a general, comforting and reassuring statement was given, like "you are a pretty smart

person"; "you certainly will pass this exam", etc.
More emphasis was laid however on the working out of a very detailed manual for rating the marks as to answers to the questions. In hoping to enhance the inter-rater reliability, the 7-point scale for the marks was reduced to a 5-point scale.

7. After the students had taken their exam, Xerox-copies were taken of their answers. That means that for each of the students there were two sheets of Xerox copies. The 35 sets of these copies, together with a copy of the manual, were handed over to Dr. Nordbeck. Then the test-sets, still sealed, were brought to the Psychological Laboratory of the University of Utrecht, by the author.
As has previously been mentioned, Dr. Vroon was also responsible for opening the test-sets and for evaluating which questions in the test-set had been associated with primary targets. A copy of this list was later handed over to the present author for further evaluation. If a statistically significant differential effect showed up, it might be thought that it was caused more by the experimenter's own ESP than by the psi-abilities of the subjects. Therefore it was considered as important to determine the inter-rater reliability. In the pilot-study (Study I), an attempt was made to determine the inter-rater reliability on a restricted number of ratings. In Study III, the inter-rater reliability was determined from all the ratings that were made. Dr. Bertil Nordbeck, who previously had given the same course at the Dept. of Psychology, Lund University, Sweden, funtioned as co-rater in both the studies.
The ratings in all three studies were carried out blindly, e.g. without the rater's knowledge of which questions in the test-set were associated with a target and which were not. It

should be observed that in all three investigations special precautions were taken when the positions of targets and non-targets within a test-set were identified. This procedure was carried out by two persons, independently.

8. Procedure of Testing.
A few more test-sets were made than were required for the expected number of students that would take the exam. They were prepared a few days before the exam was given. At the beginning of the test the test-sets were more or less unsystematically spread among the students sitting in the class-room.
The persons who have functioned as invigilators of the exam have all been personnel at the Department of Psychology, and have not been involved in the experiments in any other way than as controllers.
In all the studies the procedure has been the same.

9. On all occasions the students have been given the following, written instruction:

> "The envelopes must not be opened: A new technique of recording your answers will be tested here. The objective of this new method is concerted with your interest; to speed up the procedure of correcting the exams in such a way that as early as within a couple of days you will be informed at least about whether or not you have passed the exam. Good luck!"

All three studies have been carried out at the Department of Psychology, Lund University, Lund, Sweden, where the author has been responsible for the course. Study I was carried out during the spring semester of 1971. Study II was carried out during the spring semester of 1972.

RESULTS

Study I
37 students took the exam. It was hypothesized that the sum total of ratings on questions in the

test-set associated with a target should be higher than the sum total of marks on the non-target questions in the test-set. This hypothesis was substantiated by the ratings made by the chief-rater. When the sign-test is used for the data obtained (See Table 2), a P value, P= .022, one-tailed will be found.
It should however be observed, that the inter-rater reliability, based on independent ratings for 15 of the 37 test-sets did not give a statistically significant correlation between the two persons' ratings. The correlation was positive and not far from being significant. It should be recognized that the co-rater was pressed for time, when he carried out his ratings. In addition the manual he used was rather a tentative one.

Study II.

35 students took the exam. The hypothesis put forth was the same in this study as in Study I. The co-rater for Study I, was at the time of evaluation of this study, too busy to take part in the experiment.
As can be seen in Table 2, the outcome of the experiment was marginally significant, with P = .045, one-tailed. The sign-test (5) did not yield a significant or almost significant result. If however, Wilcoxon's matched-pair test for related samples is used, the mentioned P-value is obtained.

Study III.

35 students took the exam. It was hypothesized that they should score lower on primary targets than on non-primary targets. Since this study was considered as rather crucial, the question of the inter-subjectivity of the experiment was considered as rather important. The hypothesis was substantiated at the .005 level of significance, one-tailed, and the inter-rater reliability both on the primary targets and the non-primary targets was highly significant. (See Table 2).

TABLE 1.

MAIN DATA FROM THE THREE STUDIES. SUM TOTAL OF MARKS FOR EACH INDIVIDUAL ON TARGETS AND NON-TARGETS. Abbreviations: T=Targets; NT= Non-Targets. The Sums in Studies I-II are based on ratings made by the Chief-rater. The Sums in Study III are combined (weighted) ratings $\frac{1}{2}(R_1 + R_2)$ made by the two raters.

STUDY I					STUDY II					STUDY III				
Ss	Sum tot.		Ss	Sum total		Ss	Sum total		Ss	Sum total		Ss	Sum total	
	T	N-T		T	N-T		N-T		T	N-T			T	N-T
1.	9.5	11.5	20	8.5	8.0	1	8.0	5.0	19	8.5	6.5	1	4.00	6.25	19	3.50	6.25
2.	8.0	9.0	21	8.5	7.5	2	9.5	6.5	20	11.0	7.0	2	5.25	5.00	20	2.50	5.50
3.	7.0	6.5	22	9.0	9.0	3	5.5	8.5	21	9.5	10.5	3	4.00	6.75	21	6.25	3.25
4.	7.5	9.0	23	8.0	8.0	4	3.5	8.0	22	8.5	6.0	4	4.75	6.25	22	6.50	5.75
5.	10.0	6.5	24	9.5	7.5	5	8.0	10.5	23	10.0	6.0	5	6.00	6.75	23	4.00	6.25
6.	9.5	7.0	25	8.5	7.5	6	10.0	11.5	24	10.0	8.0	6	6.50	6.50	24	4.25	7.25
7.	11.0	5.5	26	5.5	4.0	7	10.0	6.5	25	9.5	7.0	7	2.25	5.00	25	4.50	5.00
8.	9.0	6.0	27	9.0	9.0	8	11.0	10.0	26	8.0	9.5	8	2.25	1.50	26	5.00	5.25
9.	6.5	8.5	28	9.0	6.0	9	8.0	8.5	27	8.5	5.0	9	6.25	8.00	27	2.50	6.25
10.	9.0	10.0	29	9.5	9.5	10	6.0	6.0	28	7.0	9.0	10	5.00	6.75	28	6.00	4.50
11.	8.5	8.5	30	8.0	6.5	11	9.0	10.5	29	9.0	9.0	11	2.00	4.50	29	6.00	7.00
12.	10.5	6.0	31	10.0	6.0	12	6.0	6.0	30	6.5	3.0	12	7.00	6.75	30	2.25	0.50
13.	9.0	7.5	32	9.5	6.5	13	11.0	7.0	31	8.5	9.0	13	4.50	6.25	31	4.50	5.50
14.	8.5	10.0	33	9.5	8.0	14	7.5	8.0	32	8.0	8.0	14	2.75	5.50	32	1.50	5.50
15.	6.5	9.5	34	7.0	8.0	15	9.5	9.0	33	8.5	8.5	15	6.25	6.75	33	3.50	4.25
16.	8.0	9.5	35	9.5	8.5	16	9.5	4.5	34	9.0	9.0	16	3.75	7.00	34	2.25	4.75
17.	7.5	7.0	36	10.0	9.5	17	9.0	8.5	35	10.0	9.5	17	4.50	5.50	35	6.75	5.75
18.	10.5	10.5	37	9.0	8.5	18	9.0	10.5				18	6.00	4.75			
19.	9.0	10.5															

TABLE 2

GENERAL OUTCOME OF THE THREE EXPERIMENTS

S T U D Y I (N= 37)	S T U D Y II (N= 35)	S T U D Y III (N= 35)
Inter-rater reliability:[+)] r_s= .29 (on targets); n.s. r_s= .43 (on non-targets); n.s. Validity (predictive): Z= 2.01;[++)] P= .022 (one-tailed)	No determination of the inter-rater reliability. (only one rater of the marks). Validity(predictive): Z = 1.70 [+++)] P = .045 (one-tailed)	Inter-rater reliability: r_s= .90 (on targets) t=11.86; P <<.01. r_s= .84 (on non-targets) t= 8.99; P <<.01. Validity (predictive): Combined ratings $\frac{1}{2}(R_1 + R_2)$: Z=2.57;[++)] $P_{comb.}$=.005 (one-tailed) (Z for the Chief-rater: 2.22 Z for the co-rater: 2.32).

+) r_s in this Table is calculated on the basis of the ratings of the total marks for each individual.

++) According to the sign-test cf. Siegel (5) p. 74.

+++) According to Wilcoxon's matched-pairs test for related samples, cf. Siegel (5) p. 79.

DISCUSSION

The results from the three studies are in several respects encouraging: even if the outcome of Study II was far from clear-cut, it was nevertheless in the expected direction. By Study III, the experimental conditions were tightened-up considerably. In addition, the variation of the approach, with its substantiated prediction, no doubt about it, strengthens the case. Even if we restrict our appraisal of the method to Study III, which would be both a conservative and a rather arbitrary way of reasoning, the outcome is such that in most other branches of behavioral sciences, the findings would reasonably motivate repeated and extended studies. One should comparatively also note that the order of size of the probable psi-effect, remains constant from one experiment to the other in this series of investigations.
It should, however, be stressed that the kind of persistancy of findings that showed up in our studies is not at all unique within experimental parapsychology. What now seems to be needed, for one thing, is that the approach be repeated by other research-workers. For another that the application of the rationale will be extended to a wider range of investigations.
One can imagine potentially fruitful approaches along lines of thought related to those of programmed instruction by means of which on the one hand the amount of information, in a very strict way can be fractioned, and on the other hand a number of alternatives of suitable clairvoyant targets can be anticipated.
Will the method help parapsychology to arrive at something like a repeatable experiment? Such a conjecture, is strictly speaking premature; still one should remain open in trying to find alternative explanations for the findings and in trying to formulate design characteristics that will have test-implications for the psi-hypothesis and features by which the hypothesis could be fairly refuted. However, what makes the author feel rather enthusiastic about the findings is, that the procedure seems to cancel

out influences on a subject's scoring behavior which usually are attributed to the subject's attitude towards ESP in general or towards the ESP-task the subject is about to take. It should be stressed once more that the subjects in these investigations have not been aware that they have taken part in an ESP-experiment. Since, in the strict sense of the word, there is no experimenter in the test situation, the method seems to come to terms with such assumed effects (or artifacts) as those related to the experimenter-subject relationship, demand characteristics, specific to the ESP-test situation and the experimenter's expectancies.etc. And still another challenging question: What role does the physical proximity of the target play? Would it work just as well if the targets were presented at various distances from the subject?

It should also be mentioned that the author is, at present analyzing how such parameters as the students study-performance, on other exams in psychology, degree of preparedness, introspective reactions towards the exam, anxiety proness, measured by the Defense Mechanism Test (the DMT), may be interrelated to the supposed psi-effect measured by the studies already presented.

Finally some words restricting the previously expressed enthusiasm The strongest limitation of, related to the main study, Study III, is, that the use of the method is that it is a very laboreous and time-consuming one. Furthermore the applicability of the approach is at present some what restricted. It should also be observed that the method would not work if a subject had a perfect knowledge of how to give completely correct answers to the question. This fact doesn't work entirely against the method, since it will give us a chance to have a closer look at how cognitive processes may be influenced by psi. A certain kind of limitation, specificly related to the main study, Study III,

is, that the use of the two types of targets, primary targets and secondary targets, (that in itself may have been a smart device), makes it impossible to tell whether the observed effect is attributable to the primary targets, the secondary targets or both. This question can be answered by repeated studies whith a slight modification of the design.
Anyhow, it is to be hoped that this new method of studying what seems to be an ESP-effect under real-life conditions will turn out to be helpful in diminishing the differences in conditions, which so far, have to a great extent characterized studies of ESP in Life versus in Lab.
At any rate, the findings offer a great deal of interest. If they turn out to be valid and repeatable they seem to have rather wide-ranging implications, both for the individual and for society.

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Response Preferences In Generating Random Choices: The Equal Chance Theory[1])

Sybo A. Schouten University of Utrecht
The Netherlands

In various types of experiments, especially in parapsychological research, subjects are requested to generate sequences of choices from a finite set of responses. In parapsychological experiments the subject (S) has to select in each trial that specific alternative from a limited number of alternatives, which is similar to the target. The target is, for instance, the alternative observed by another S in a different room. While choosing, S knows that the sequence of the targets is random. It might be expected that as a consequence the sequence of responses is random as well. However, it is well known that in general Ss are unable to generate a random sequence of choices. This holds for experiments in which Ss are explicitly asked to generate a random sequence of choices (see for a review Tune, 1964b; Wagenaar 1972) as well as for parapsychological experiments.
When instructed to generate a random sequence, Ss tend to alternate their responses, as a consequence the sequence shows too few repetitions. In addition they tend to follow certain patterns which result in so-called higher order effects. A k^{th} order effect is to be defined as a dependency between a choice and

1) The author expresses his thanks to Luc Heyendaal and Folke Stuivenberg who acted as experimenters.

the k^{th} previous choice. The tendency to alternate results in a first order effect, since alternation creates a dependency between a choice and the immediate previous choice.
Mittenecker (1953, 1958) has shown that the distribution of recurrency distances - a recurrency distance is the number of chosen alternatives between a choice and the first repetition of that choice - based on the response sequence of the Ss differs significantly from the theoretical distribution. He concluded that Ss tend to balance the frequency of each alternative within a limited number of calls. According to him this phenomenon is responsible for the finding of too few repetitions.

An analysis of a number of (yet unpublished) parapsychological experiments carried out by the author revealed the same phenomena. In all these experiments the possible alternatives were presented visually. Ss responded by touching the chosen alternative or by pressing a button placed next to the alternative. Although individual differences were found, it was observed that Ss tended to avoid repetition of the previous choice and that they tried to balance the frequencies of the alternatives within a limited number of calls.
Moreover, when the alternatives were presented visually and arranged in a certain pattern sometimes a zero order effect was found. A zero order effect indicates the tendency to call the alternatives with different frequencies. These zero order effects appeared to depend mainly on the position of the alternatives. If Ss should indicate each choice by pressing a button, alternatives within easy reach are called more often than others. If each alternative can be reached with equal effort and zero order effects are still found, it appears that centrally placed alternatives are called more often than those at the extremities of the pattern.
An analyses of the relation between the tendency to avoid repetitions and the tendency to balance

the frequencies of the alternatives within a limited number of calls showed, that in the case of short response sequences ($n < 75$) no relation between both phenomena existed. This implies that Mittenecker's explanation of the repetition effect can not be entirely correct.

One could argue that response preferences in parapsychological experiments in which Ss have to select in each trial one alternative from a limited number of alternatives, results mainly in three effects; a possibly zero order effect, a tendency to avoid repetition of the previous choice and a tendency to balance the frequencies of the alternatives within a limited number of calls. Higher order effects can be explained as a consequence of the balancing tendency. This tendency implies that on average a symbol is repeated after all other symbols are called. As a consequence a lack of repetitions will be found in orders lower than the order associated with the average recurrency distance. No indications were found that other higher order effects existed, except those caused by the balancing tendency.
These response preferences are caused by tendencies, which are found to be rather common to all Ss participating in the experiments. All analyses were based on distributions of the number of repetitions in each S response sequence. This implies, that these tendencies are part of a choice strategy which is employed by the majority of the Ss.

These data may be interpreted as follows. It is assumed, that for the Ss the concept of randomness is equivalent to the idea of "equal chance" for all alternatives, combined with the idea of "not systematically". Furthermore it is assumed, that the Ss do not try to recall many of their previous choices and consequently remember only a small number of previous choices. Equal chances for all alternatives is interpreted by the Ss as equal frequencies for all alternatives. This implies, firstly, that none

of the alternatives is to be called more often than the others, and secondly, that none of the alternatives is to be forgotten. The first requirement can be fulfilled easily. Considering the small number of previous choices which are remembered by the subject, repetition of a certain choice will give the impression of favoring that specific alternative.
Therefore the S tends to avoid repetitions. Realizing the second condition is more difficult. Assuming that Ss do not try to recall a number of previous choices, this can be accomplished by selecting and following choice sequences based on the visually presented pattern of symbols in such a way, that within each sequence all alternatives are called about once. So the response series is actually composed of a number of small sequences, each of which comprises of about all alternatives. Hence it is found that the frequencies of the alternatives are more or less balanced within a limited number of choices, while the length of this number of choices will mainly depend on the number of alternatives. As the main response preference effects are supposed to be caused by the idea of equal chance for all alternatives, this theory is called "equal chance theory".
It is important to note that this strategy is based on selecting sequences through the pattern of symbols, which results in a number of choices still to be made at the moment of selecting a sequence. For instance when the symbols are arranged in a circle S might decide at one moment to start with a specific symbol and then to proceed clockwise till he arrives at this symbol again. Here lies the main distinction with those theories which try to explain non-randomness of response series as a consequence of the limitation of memory (see for instance Tune 1964a). These theories assume that S chooses only one symbol at the time and only looks backward when determining his choice. This requires the recall of the individual previous choices.

It is assumed that "non systematic" is interpreted by the Ss as not repeating the same sequences of choices. This means, that in selecting sequences they will tend to avoid repeating the same sequence.
As is explained before in choosing sequences Ss will mainly use the pattern or in different words the position of the symbols, instead of their contents. Now it is to be expected that symbols placed in the centre have more chance to become part of a sequence than alternatives placed in extreme positions. This can cause a zero order effect in spite of the aim of the strategy of the Ss to equalize the frequencies and thus to minimize the zero order effect. This apparent contradiction can be explained as follows.
Ss try to balance the frequencies by applying the aforementioned strategy. In general they will succeed rather well but naturally slight differences between frequencies of alternatives will remain. However, between Ss these differences are not randomly distributed over the symbols. As a consequence of the common strategy those differences are likely to be shown by the majority of the Ss as differences between the central and extreme positions. This may result in a significant zero order effect which is to be considered as a by-product of the employed strategy. Therefore this zero order effect can not be considered as a "preference"of the Ss for specific symbols. On the contrary, it is caused by the fact that Ss try to show no preference for any of the symbols.

The purpose of the present experiment is to investigate whether the given interpretation of the response preference phenomena is valid. The main phenomena are explained by assuming that the Ss strategy is predominantly influenced by a combination of a certain concept of randomness - equal chance for all alter-

natives and non systematic - and the limitation that they can only realize this concept within small choice sequences. It may be expected that this explanation holds also for non-parapsychological experiments, provided that the Ss are asked to choose as randomly as possible. Therefore, the experiment carried out is not of the parapsychological type, Ss only being asked to choose randomly.

Hypotheses

Three types of hypotheses should be distinguished: hypotheses related to the data on which this theory is based, hypotheses related to the assumptions introduced when setting up this theory, and hypotheses related to predictions which follow from this theory.
As the main phenomena to be expected are the tendency to avoid repetition and to balance the frequencies of the alternatives, the data should show:

1. The distribution of the number of repetitions in the response sequence of each S differs significantly from the theoretical distribution. The top of the distribution will be shifted to lower values on the number of repetitions.
The extent to which the Ss equalize the frequencies of the alternatives within a limited number of calls is expressed in a S-score. A low S-score indicates a high degree of balancing of frequencies. So the data must also show that:
2. The distribution of the S-score values of the response sequence of each S differs significantly from the theoretical distribution. The top of the distribution will be shifted to lower S-score values.
Although "alternating" and "balancing" are part of the same strategy of the S, both are in principle independant. The finding of too few repetitions is not a direct consequence of the phenomenon that Ss balance the frequencies of the alternatives. Would that be the case, a close relationship between both effects has to be found. It is to be expected, that no close relationship

will show. Only a weak relationship is possible, because especially in the case of a low number of alternatives a high value of S will increase the mathematical probability for the number of repetitions. Hence:
3. There is no relationship between the number of repetitions in, and the S-score value of a response sequence.

When the alternatives are presented visually and the Ss are to indicate their choices by touching the alternatives and if there are none or negligible differences in the effort to reach all alternatives, the zero order effects will depend mainly on the position of the alternatives. Therefore:
4. When the alternatives are arranged in such a pattern, permitting distinguishing between alternatives placed in the centre, and alternatives placed at the ends; and if zero order effects are found, the frequency of the alternatives placed in the centre will be higher than those of the alternatives placed at the ends.

When it requires more effort to reach some alternatives, it will appear that:
5. Alternatives, requiring more effort to be reached, will have lower frequencies than the other alternatives.

When setting forth this theory, some assumptions had to be made to fit the theory to the data. One of these assumptions is, that the Ss will not try to recall a large number of previous calls, for instance in order to equalize the frequencies of the alternatives, but that they apply strategies not depending on the retention of a large number of previous calls. It follows from this assumption that higher order effects can not be explained by assuming a direct influence from the previous choices. It may thus be expected that:
6. When Ss are unexpectedly asked to indicate their previous choices, they will only be able

to recall a limited number of choices; this number being smaller than the number necessary to explain higher order effects.

As the Ss use mainly the position of the alternatives when they apply their strategy it is to be expected, when Ss are asked to indicate the position of their previous choices as opposed to the content (in the present experiment colour) of their previous choices, that:
7. Ss will recall the position of their previous choices better than their content.

An important rôle is played by the assumption, that the main components of the Ss concept of randomness are "equal chance or equal frequencies for all alternatives" and "not systematic". Therefore:
8. When Ss are asked to mention the most relevant aspects of the concept" to choose random", it is to be expected that they will mention these two aspects most frequently.

When discussing the explanation of zero order effects, it was assumed that alternatives, placed in the centre, were called more frequently than the others, because the likelihood of becoming part of a subsequence is higher for these alternatives. This implies, that in selecting a way through the alternatives, Ss will tend to go by the centre and not by the extremities of the pattern, formed by the alternatives. Thus:
9. When Ss are asked to draw a random line in a square from one corner to the opposite corner, it is to be expected, that this line will cross the central part of the square more often than the noncentral part.

From this theory predictions can be made with respect to the effect of variables like the number of alternatives, introducing a double task, offering information about previous choices, etc. In general it can be put forward,

that conditions facilitating the S to reach his objectives "equal frequencies for all alternatives" and "not being systematic", without creating conflicts between both objectives, will enhance non-randomness. Conditions creating a conflict between both aims or impeding the application of certain strategies, will result in more random response sequences.
Ss will try simultaneously to balance the frequencies of the alternatives and to avoid being systematic. The latter means, that they will try to avoid repetition of identical subsequences of choices. It is understandable, that both aims will clash when choosing from a very low number of alternatives, since the number of possible subsequences is rather low. In order to avoid repetition of identical subsequences Ss become more or less forced to accept subsequences in which repetition of identical alternatives occurs. However, this will increase the difficulty in balancing the frequencies.
For example, when choosing from two alternatives most Ss will start with a pattern like ABA, but are then forced to show repetition in order to avoid the pattern ABABAB... These repetitions hamper the application of a simple strategy to balance the frequencies, and this can only be accomplished by regularly shifting the "accent" from one alternative to the other. But, as in this case they can not use the position of the alternatives, and in view of the fact that an external reference system is lacking, the results of this strategy will be rather unreliable. The situation is different when choosing from a relatively large number of alternatives. The number of possible combinations of two successive different alternatives is so large, that Ss can avoid repetitions of identical alternatives without being forced to repeat identical subsequences. Therefore:
10. It can be expected that the effect of the tendency to avoid repetition of identical alternatives will increase with the number of

alternatives.
In addition, in order to ensure that no alternative is "forgotten", Ss can use the position of the alternatives to select subsequences through the pattern of alternatives, covering more or less all alternatives. With a large number of alternatives, the number of these subsequences increases, which enables the S to use a number of different subsequences successively without becoming "systematical". Therefore it is to be expected:
11. The effect of the tendency to balance the frequencies of the alternatives within a limited number of choices will increase with the number of alternatives.

As mentioned before, one of the assumptions is that Ss recall not more than a very limited number of previous choices. Presenting information about previous choices will then be used to balance the frequencies more effectively. This is especially relevant in the case of a low number of alternatives, because the fewer the number of alternatives, the more difficult it is to balance the frequencies. Thus it can be expected that:
12. Presenting information about previous choices will increase the effect of the tendency to balance the frequencies of the alternatives. And:
13. This effect will be stronger for small numbers of alternatives.

As regards the tendency to avoid repetition of the previous call the effect of presenting information will be different. In the case of a relatively large number of alternatives, Ss can choose successively different subsequences to balance the frequencies and avoid repetition of identical alternatives without creating conflicts.
Presenting information will thus be used to improve frequency balancing, but it will hardly effect the number of repetitions, since with both strategies no conflict is expected.
The situation is different in the case of a small number of alternatives. Presenting information

will result in the application of an entirely different strategy to balance the frequencies. Ss will try to balance the frequencies within the number of previous choices showed. For instance, when offering six previous choices in a dual-choice task, the S will choose in such a way, that the frequencies are balanced within these 6 choices. This is for him a much simpler strategy than the strategy he is forced to apply when lacking this information. This strategy, however, will result in a decrease of the number of repetitions of identical alternatives, since the number of possible subsequences with a small number of repetitions of identical alternatives in which the frequencies of both alternatives are more or less equal is rather large. Consequently Ss can choose different successive subsequences, in which the alternatives are balanced without creating a conflict and becoming too systematical. Hence they will be less forced to repeat identical alternatives, Therefore:
14. Offering information will hardly effect the number of repetitions of identical alternatives in the case of a relatively large number of alternatives, but:
15. will result in a decrease of the number of repetitions of identical alternatives in the case of a relatively small number of alternatives.

Offering a task between the choices will hinder the application of strategies in order to balance the frequencies and will thus result in more random sequences. In the case of a relatively small number of alternatives it is rather difficult even without carrying out another task to balance the frequencies. So in thisecase the effect of carrying out another task will be rather weak. On the other hand, a strong effect can be expected in the case of a relatively large number of alternatives. The task will increase the difficulty in selecting and following subsequences, covering about

each alternative, and consequently the likelihood increases that alternatives are "forgotten".
Therefore:
16. Introducing another task will hardly effect the result of the tendency to balance the frequencies in the case of a small number of alternatives,
but:
17. Introducing another task will effect considerably the result of the tendency to balance the frequencies in case of a large number of alternatives. It is to be expected that the sequence will become more random.

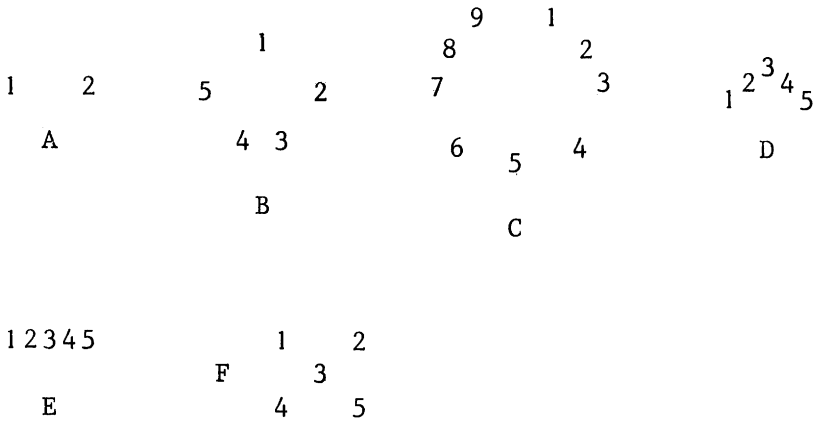
As regards the tendency to avoid repetition of identical alternatives is the relation with the number of alternatives becomes reversed. With a large number of alternatives, already showing relatively few repetitions, hardly any effect is to be expected. However, in the case of a small number of alternatives, fewer repetitions will appear. In the case of a small number of alternatives Ss are more or less forced to include more repetitions, in order to avoid being systematic. By adding a task, it will become harder for the Ss to notice this effect. Therefore it can be expected that introducing a task will result in fewer repetitions.
18. Introducing a task will hardly effect the tendency to avoid repetitions in the case of a large number of alternatives
but:
19. will strengthen the effect of the tendency to avoid repetitions in the case of a small number of alternatives.

The main theme of the theory is the concept of equal chance for each alternative. So it can be expected that instructing Ss to choose non randomly (not to care about equal chance for each alternative) will effect both main tendencies: the avoidance of repetitions and the balancing tendency; and will result in more random sequences.
Therefore:

20. Instructing the Ss to choose non randomly and not to care about equal chances for all alternatives will result simultaneously in more repetitions of previous calls and less balancing of the frequencies. Zero order effects may be explained as a by-product of the balancing tendency, depending on the relative difference in position (central or extreme) of the alternatives. Thus in the case of relatively equal positions of the alternatives no zero order effects are to be expected.
21. If all alternatives are situated in a relatively equal position with respect to each other, no zero order effects will appear. This hypothesis has some consequence for the experiment; because zero order effects will slightly influence the degree of balancing (especially in the case of few alternatives). A fair comparison between conditions with regard to the balancing effect requires, especially when different numbers of alternatives are involved, relatively equal patterns of the alternatives. The best way to fulfil this requirement seems to be to apply patterns, in which each alternative has a relatively equal position with respect to the others.

METHOD

Patterns of alternatives



In A, B and C the alternatives consisted of coloured cards, size 6 x 7,5 cm. In D, E and F the cards were of equal size, but these cards showed simple line drawings. In A, B and C the same numbers refer to the same symbols. In D, E and F the symbols applied were put into different position (central or extreme) in each pattern. The exact relation between number and symbol is given in table 6.
The size of the patterns was: A: distance between 1 and 2 is 30 cm. B: diameter of the circle is 30 cm. ; C: diameter of the circle is 30 cm. ; D: length of the base of the triangle is 37 cm., length of the other sides 32 cm. ; E: distance between 1 and 5 is 50 cm. ; F: length of side of the square is 22 cm.

Display

The patterns were placed on a display (50 x 60 cm.), the angle of the display with the horizontal plane was about 60 degrees.
Ss indicated their choices by pointing out the chosen alternative on the display with a stick of 30 cm. length. Ss were seated in front of the display approximately in such a position that they could reach all alternatives with about the same ease when using their right hand (for left-handed people their left hand).

Conditions

The experiment consisted of 14 conditions.
The order of the conditions remained fixed for all Ss. The conditions composed of I: pattern A; II: pattern B and recall ; III: pattern C; IV: pattern D; V: pattern A and task; VI: pattern E; VII: pattern C and information ; VIII: pattern F ; IX: pattern A and information; X: pattern C and task; XI: pattern B lefthanded and recall; XII: drawing a random line; XIII: answering the questions "what are for you the most important aspects of randomness" ; XIV: pattern B with instructions for choosing non randomly.

The conditions I, II and III should be considered as the main conditions, in which no variables are introduced. In these conditions all alternatives were given approximately relatively equal positions by arranging them in a circle. In conditions IV, VI and VIII, the alternatives were arranged in different positions, the patterns D, E and F containing respectively 2, 3 and 1 alternative(s) placed in the centre.
The conditions V, VII, IX and X are focused on studying the effect of introducing a task or information, the scores in these condition can be compared with those in the conditions I and III. In condition XIII Ss were instructed to point out their choices with the other, at most times left, hand. It was assumed that then the alternatives, situated furthest away from this hand, would be more difficult to reach. Condition XIV was put at the end of the experiment in view of the nature of the variable investigated. Because of the time lag between this condition and condition II it was decided, to use pattern B for the condition, in which the effect of pointing out the alternatives with the left hand was investigated. So the effect of the instruction to choose non randomly could be found by comparing the results of condition XIV with the results of the immediately previous condition in which the S had to choose, as in both conditions the same pattern was used.

Task

Ss were given a paper with a number of lines, each line consisting of 25 small, irregular patterns of 3, 4 or 5 dots. They were requested to find and circle each pattern with 4 dots (Bourdon Wiersma test) and to complete as much lines as possible.

See next page

Information

Ss were presented visually their last 6 choices by putting identical cards as the chosen ones in a holder on top of the display (about eye-level). After each choice the chosen symbol was added and the card, similar to the seventh previous choice, was automatically removed.

Drawing a line

Ss were presented a square with sides of 20 cm. and requested to draw a random line from the left under-corner to the right upper-corner. A straight line between both points was not permitted.

Recall

After finishing the conditions II and XI, Ss were asked to indicate their last choice; subsequently the choice previous to the last choice, etc. Recall was asked under the following conditions.
a) position: the display was immediately after finishing the condition covered by another display, on which 5 white cards were fixed, and on which the positions corresponded with pattern B. So Ss had to indicate their recalled choices by pointing out their positions.
b) colour: the display was covered and in front of the S, 5 cards with the colours of the alternatives were placed in a random order. Ss were requested to indicate their recalled choices by pointing out their colours.
c) display (position and colour): Ss indicated their recalled choices by pointing out these alternatives on the original display.
The conditions a, b and c were randomised over all Ss and over the conditions II and XI.
If S could not recall a choice he was not asked to guess.

Length of the sequences

The length of the sequences in each condition amounted to 25 choices.

Tempo

Ss answered after a click of the metronome. The pace amounted to 4 sec., except for conditions V and X, for which this pace was a bit too fast because it did not allow Ss time enough to concentrate on the task. In these conditions the pace was set at 6 sec.

Subjects

59 Ss, average age about 20 years, took part in the experiment.

Instructions

general: In this experiment the topic under investigation is what is known as randomness. A display with a certain number of cards on it will be placed in front of you. Each time you hear a click you have to choose as randomly as possible one of these cards. You have to choose 25 times, the experimenter will tell you when to stop. It is crucial that you choose as randomly as possible. So do not choose a card because, for instance, you like the colour, but choose in such a way, that according to your opinion the choices are random.

when introducing the task: Now you have to perform simultaneously another task, consisting of ..... Choose again as randomly as possible. This is important. In this case you may indicate your choices by using your pencil.

when offering information: Because it might be useful to you in order to choose as randomly possible, we shall show you each time your 6 previous choices. You should inspect them between the clicks of the metronome. Chose eventually with the help of this information as randomly as

possible.

when drawing a line: Draw a random line from A (under left) to B (upper right). This line can be as long or as short as you wish. Only a straight line between A and B is not allowed. Draw this line as randomly as possible.

when asking aspects of randomness: Can you tell me what you consider as the most important aspects of random, when you have to choose as was asked of you? What did you take into account when choosing?

when asked to choose non randomly: Now we shall give you some information. The most important aspect of randomness is, that in each trial all alternatives have an equal chance of being chosen, so that the choices are unpredictable. Now you are asked not to choose randomly anymore. You do not have to bother about equal chances for all alternatives. So do not mind when some cards are chosen more often or some less often than the others. However, do not follow a special system, for instance, by intentionally not calling a certain card. While choosing you must be indifferent to the idea, that some cards are called more often or less often than the others.

SCORING

zero order effects ; For each S, the alternatives for the conditions of importance are ranked according to their frequencies. On these data Friedman s two-way analysis of variance is applied.

repetition of previous choices : If the zero order effect is not excessive, the number of repetitions can be assumed to be distributed according to the binomial $(p + q)^{24}$: ($p = 1/a$, a being the number of alternatives).

The distribution of the number of repetitions over the Ss is compared with the expected distribution by the Kolmogorov-Smirnov one-sample test.

The balancing effect: For each number of alternatives involved, a S-score is defined, indicating the extent to which the frequencies of all alternatives are equalised within the 25 choices.

$a = 2 : S_2 = \sum_{i=1}^{2} \left| f_i - 12.5 \right|$, f_i being the frequency of calling alternative i.

$a = 5 : S_5 = \sum_{i=1}^{5} \left| f_i - 5 \right|$

$a = 9 : S_9 = \sum_{i=1}^{9} \left| f_i - 3 \right|$

The expected distributions for each S-score are for each number of alternatives based on 10.000 computer-generated random sequences of 25 choices. These distributions are given in table 1. Empirical and expected distributions are again compared with the help of the Kolmogorov-Smirnov test.

Table 1: Expected cumulative S-score distributions for sequences of 25 choices.

S_2	a=2 Maximum possible deviation is .687				
S-score	1	3	5	7	≥ 9
C_E	.313	.578	.773	.893	1.000

S_5	a=5 Maximum possible deviation .998							
S-score	0	2	4	6	8	10	12	≥ 14
C_E	.002	.038	.171	.413	.671	.859	.952	1.000

S_9	a=9 Maximum possible deviation .999						
S-score	2	4	6	8	10	12	≥ 14
C_E	.001	.012	.074	.238	.494	.726	1.000

Other analyses ; In nearly all other analyses the Wilcoxon matched-pairs signed-ranks test for related samples is applied.

RESULTS and DISCUSSIONS

The results are discussed on the basis of the hypotheses.

I. The distributions of the number of repetitions over the Ss in the conditions I, II and III, together with the D and the $D_{crit.}$ values, indicating the extent of the difference with the expected distributions, are given in table 2.

Table 2: Cumulative distributions of the number of repetitions in conditions I, II and III.

Condition I	a=2		D= .216		D_{crit}=0,21	
N repetitions	≤ 7	8	9	10	11	
Cf	.102	.136	.254	.356	.610	
N repetitions	12	13	14	> 15		
Cf	.797	.864	.949	1.000		
Condition II	a=5		D= .597		D_{crit}=0,21	
N repetitions	0	1	2	3	4	≥ 5
Cf	.271	.475	.712	.831	.915	1,000
Condition III	a=9		D=.602		D_{crit}=0,21	
N repetitions	0	1	2	>3		
Cf	.661	.814	.932	1.000		

All the differences are significant, and as the top of each distribution is shifted in the direction of lower values of the number of repetitions, it can be concluded that the Ss tended to avoid repetition of the previous choice. This result confirms hypothesis 1.

2. The S-score distributions for condition I, II and III, together with the statistics of the Kolmogorov-Smirnov test, are given in table 3.

Table 3: Cumulative S-score distributions in conditions I, II and III.

Condition I	a=2	D= .296			D_{crit}= .21
S-score	1	3	5	> 7	
Cf	.559	.874	.915	1.000	

Condition II	a=5	D= .592			D_{crit}= .21
S-score	0	2	4	6	8 > 10
Cf	.068	.390	.763	.932	.966 1,000

Condition III	a=9	D= .677			D_{crit}= .21
S-score	2	4	6	8	> 10
Cf	.051	.203	.695	.915	1.000

The shape of the distributions show, that in each condition Ss tended to balance the frequencies of the alternatives to a significant degree.
This result confirms hypothesis 2.

3. Table 4 shows for each S-score the mean value and the sd of the number of repetitions of the response sequences, having that specific S-score value. Theoretically it is to be expected, particulary in the case of a small number of alternatives, that there is a slight positive correlation between the S-score and the number of repetitions. This phenomenon is due to the fact that a high S-score is associated with a reduced number of different elements in a sequence so that the probability of a repetition will be increased. In the case of 9 alternatives table 5 presents an example of this phenomenon. The data are based on 5000 computer-generated random sequences of 25 choices.
In view of this argument and considering the size of the sd values, it may be concluded, that there is no distinct relationship between the S-score value of a response sequence and the number of

repetitions in that sequence. Hence it is not possible to explain the avoidance of repetitions as a direct consequence of the tendency to balance the frequencies or vice versa.

Table 4: Mean number of repetitions and sd in the number of repetitions of all sequences with a specific S-score value in conditions I, II and III.

Condition I	a=2				
S-score	1	3	5	7	
M repetitions	10.7	10.9	12.0	11.2	
sd	2.39	1.92	3.08	1.33	
Condition II	a=5				
S-score	0	2	4	6	> 8
M repetitions	0,75	1,78	2,18	2,30	2,75
sd	0,83	2,59	1,85	2,41	0,83
Condition III	a=9				
S-score	2	4	6	8	> 10
M repetitions	0.67	0.44	0.75	0.77	1.20
sd	0,94	0.68	1.61	0.80	2.40

Table 5: Relationship for 9 alternatives between S-score and mean number of repetitions for sequences of 25 random choices.

S-score	2	4	6	8	10	12	14
M repetitions	1.86	2.04	2.18	2.27	2,46	2.71	3.10

4. It is possible to classify the alternatives in the patterns D, E and F into alternatives placed in the centre, and alternatives placed at the extremities. The patterns are chosen in

such a way that the ratio of both types of alternatives ranges from 1:4 (F) to 3:2 (E). It was predicted that if zero order effects would be found, frequencies would be higher for the centrally placed alternatives.
For each S the alternatives are ranked according to their frequencies. For each pattern, the sum of these rankings, together with the associated Chi-square values, are given in table 6.

Table 6: Sum of ranks for the alternatives in condition IV, VI and VIII.

Alternative Symbol	Condition IV	Pattern D				
	1 triangle	2 rhomb	3 house			
Sum Ranks	161	192	164			
Alternative-symbol	4 square	5 circle				$Chi^2=5,2$ P=ns
Sum Ranks	187	181				
Alternative symbol	Condition VI	Pattern E				$Chi^2=22,3$
	1 square	2 triangle	3 circle	4 house	5 rhomb	
Sum Ranks	141.5	204	190.5	198	151	P<.01
Alternative symbol	Condition VIII	Pattern F				
	1 rhomb	2 triangle	3 square	4 house		
Sum Ranks	165.5	175.5	207	184.5		
Alternative symbol	5 circle					$Chi^2=11,5$
Sum Ranks	152.5					P <.025

The data confirm the hypothesis. Even in the case of condition IV, in which the zero order effect showed to be not significant, the alternatives in the centre were ranked the highest.
The assumption that the position and not the content of the symbol is responsible for the zero order effects, may be illustrated by the following examples.

The symbols "triangle" and "house" were both ranked the lowest in pattern D, but "triangle" was ranked the highest in pattern E, while "house" was about ranked the highest in the patterns E and F. The symbol "square" was ranked the lowest in pattern E, but the highest in pattern F. An alternative explanation for the finding of zero order effects, depending on positions, is that Ss have a "natural" preference for certain positions independent of the sequential response bias. The distribution of the first choice should present "evidence" for this effect.

For the patterns A,B,C,D,E and F (Conditions I, II, III, IV, VI and VIII respectively) the distributions of the first selected alternative by each S were investigated and tested by a Chi-square. A significant result was found with pattern A ($X^2 = 6.12$; df = 1 ; $P < .02$) and with pattern F ($X^2 = 13.6$; df = 4 ; $P < .01$). With pattern A Ss showed a preference for alternative 2 (green, placed at the right side), but as will be shown in table 11, this effect is not found in the total results of the series. With pattern F the distribution showed the same trend as the ranked distribution as given in table 6. This result is the only indication that such a "natural" preference might exist for centrally placed alternatives.

However, in the case of pattern A; the preference shown in the first choice did not result in a zero order effect. The results of the patterns D and E indicate that a zero order effect was found with 25 choices whereas no preference was observed in the first choice. This finding indicate that the eventual effect of a "natural" preference is only of minor importance.

5. In condition XI (pattern B lefthanded) Ss were asked to point out their guesses with their left hand (for lefthanded people with the right hand). The position of both the S and the display remained the same as in the previous conditions. The Ss were situated such as to ensure, that for the right hand all

alternatives were within equal reach. It was assumed that in the case of using the left hand, it would be more easy to reach the alternatives at the left side (4 and 5), than at the opposite side. (2 and 3). Consequently, a zero order effect can be expected, favouring frequencies of the alternatives 4 and 5. As pattern B was used, five alternatives in a circle, each alternative having roughly an equal position as regards the others, no other zero order effects were expected. The data are presented in table 7.

Table 7: Sum of ranks for the alternatives in condition XI.

Condition XI

alternative	1	2	3	4	5	X^2	P
Sum of ranks	178	160.5	181	168.5	197	5.2	n.s.

The data do not support the hypothesis since the zero order effect is not significant. Comparing for each S the frequencies of the alternatives 4 and 5 versus 2 and 3 per S (for lefthanded Ss the reversed comparison) it appears that a weak trend in the predicted direction exists (Wilcoxon; z=1,83; P=.05 one-sided).
Probably the negative result is due to the fact that the Ss used a rather long stick in pointing out the alternatives, so actually the difference in effort in using right or left hand have not been very strong.

6. Recall of previous choices was unexpectedly asked in condition II and XI. Ss indicated the recalled choices by pointing out the position, the colour, or the original display. The results of these three conditions are given in table 8.
The mean values show, that generally Ss recall at most only their previous two choices. Consequently higher order effects can not be explained by assuming a direct influence from recalled previous choices, and this result confirms hypothesis 6. Since Ss were not forced to indicate a specific number of previous choices an alternative explana-

Table 8: Mean number and sd of recalled previous choices under three conditions.

	Colour	Position	Display
$N_{subjects}$	44	44	28
M_{recall}	0.86	0.95	1.54
sd	1.04	1.13	1.05

tion for these results can be that they did not take the effort to reproduce more than a very limited number of previous choices. This interpretation, however, is not very convincing. It is true that forcing the Ss to give more reproduction would have increased the number of correct responses, but the data show, that even without this obligation the number of incorrect responses is about 37% of the total number of given responses. Moreover, it is important that if the Ss really judged it to be too much effort to try to reproduce more of their previous choices, it is highly inlikely that they would take that effort with each choice while performing their choice task. Therefore it is concluded, that these data clearly indicate, that higher order effects can not be explained only in terms of recall.

7. Since the position of the alternatives plays an important rôle in the choice strategy, it was expected that indicating the position of the recalled choices would yield a higher number of recalled items than indicating the colour. Although the mean number of recalled choices when indicating positions is slightly higher than the mean value for indicating colour, the difference is not significant (Wilcoxon; $z = 0,79$; n.s.) so that hypothesis 7 is not confirmed. It is possible, that this negative result is due to the fact, that Ss when giving recall in doing so have already used the possibility to transform position to colour and vice versa.

8. Ss were requested to define the main aspects of the concept of randomness and it was expected, that the concepts "equal chance or frequencies" and "not systematic" would be mentioned most often. The statements given by the Ss were classified independently by a judge, not familiar with the theory or the aim of the question, and the author. The following classification has been employed.
a) Equal chance or frequencies. For instance: "Do not prefer one alternative more than the other", "I tried not to miss an alternative".
b) Not systematic. For instance: "Must be unpredictable", "No regularity", "Do not follow a system".
c) Other strategies applied. For instance: "Do not choose all symbols with equal frequencies", "I chose specific combinations of colours or symbols".
d) Statistical concepts or concepts strategically not applicable. For instance: "Each choice has to be independent of the previous choice", "One has to choose according to the laws of probability", "One has to choose intuitively or not consciously".
e) Not relevant answers.
Each S gave on average about two statements. The results of the classification in percentages are given in table 9.

Table 9: Classification in percentages of statements about the concept "random" by an independent judge and by the author.

Condition XIII

Class	a	b	c	d	e
judge	35	29	24	8	4
author	33	19	29	10	9

Both judges classified over 50 per cent of the statements in "a" or "b", while the number of statements classified in "d" is 10 per cent or less. This result confirms hypothesis 8, the concepts of "equal chance or frequencies" and "not systematic" play an important role in the Ss concept of randomness.

9. Ss were requested to draw a random line from one corner of a square to the opposite corner, and it was predicted that they would show a tendency to cross the central part of the square. To analyse this, the diagonal perpendicular to a straight line between the two points to be connected was divided into a central part of 14,0 cm. and two extreme parts each of 7,1 cm. For each S the number of crossings of the central and the extreme part of this diagonal was counted.
It was found, that in total the central part was crossed 85 times, while the extreme part was crossed 37 times only. Fifty Ss favoured the central part - which means that each line crossed the centre more often than the extreme - against 9 Ss favouring the extreme part of the diagonal. The sign test shows this difference to be significant ($z = 5,21$; $P < .01$), so hypothesis 9 can be considered as being confirmed. This finding supports the assumption that when Ss select subsequences in a pattern of visually presented symbols, they will tend to select sequences which cross the centre. Hence the centrally placed symbols have more chance to become included in such a sequence than the others.

10. When the mean number of repetitions in the conditions I, II and III are expressed as a percentage of the expected number of repetitions, it is found that this percentage decreases when the number of alternatives increases. The data are given in table 10.

Table 10: Mean number of repetitions expressed as a percentage of the expected number of repetitions.

Condition	I (a=2)	II (a=5)	III (a=9)
$M_{repetitions}$	10,9	2,0	0,74
% E	90,8	41,7	27,7

In addition, table 2 shows that the differences between the emperical and expected distributions, as expressed by the size of the D values, increase with the number of alternatives. Hence the data confirm the hypothesis, that the effect of the tendency to avoid repetition increases with the number of alternatives.

11. Table 3 shows, as the Davalues increases with the number of alternatives, that the effect of the tendency to balance the frequencies increases with the number of alternatives. It should be taken into account, however, that the maximum possible deviation between the distributions (see table 1) is not equal for the conditions. This result confirms hypothesis 11.

12 and 13. In condition VII (9 alternatives) and IX (2 alternatives), the S was presented information about the 6 previous choices. The effect of this variable on the tendency to balance the frequencies was investigated by comparing the S-score values found in these conditions with those found in the conditions III and I respectively. The difference which were in the expected direction were tested by means of the Wilcoxon test. The difference appeared to be marginal significant for IX versus I ($P = .02$; one-sided), but not significant for VII versus III. However, the fact that the influence of information on the S-scores appears to be weak is not surprising, in view of the already very low S values in the conditions I and III. This can be illustrated as follows. The marginally significant effect of IX versus I is actually based upon $N = 34$, instead of $N = 59$, the rest of $n = 25$ being tied observations. From these 25 zero differences 21 showed to be of the type 1-1, meaning that in both conditions the S-score received the lowest possible value. Hence the explanation for the rather weak effect is that most Ss showed a strong balancing effect to such a degree in the first condition that adding information could hardly strengthen this effect. The same explanation is applicable to the comparison of the conditions III and VII. The S-score distribution in condition III already differs from the expected distribution withe extreme value of $D = 0.68$.

In fact, the strategy to balance the frequencies applied by the Ss in I and III, has been efficient to such a degree (for instance in condition I,56 % of the Ss got the lowest S-score possible) that a strategy based on information could hardly improve the results. The fact that the results are better in the case of a low number of alternatives is not surprising, since it has been stated before that balancing the frequencies is more difficult the fewer the number of alternatives. So it was to be expected that the effect of an improved strategy, the application of which hardly does depend on the number of alternatives, will produce the strongest effect in the case of a low number of alternatives.

14. The effect of information on the tendency to avoid repetition in the case of a relatively large number of alternatives is investigated by comparing the number of repetitions in the conditions III and VII by means of a Wilcoxon test. The result showed to be not significant ($z = 0,12$), which confirms hypothesis 13.

15. This hypothesis was also confirmed. Information appeared to have a strong effect upon the number of repetitions in the case of two alternatives, the effect is in the expected direction. The number of repetitions decreased significantly when presenting informations (Wilcoxon; $z = 3,78$; $P << .01$).

16. Performing a second task did not influence the effect of the tendency to balance the frequencies in the case of a small number of alternatives. Compared are the S-scores of condition I versus V (Wilcoxon; $z = 0.64$). This result confirms hypothesis 16.

17. The effect of introducing a task in the case of a relatively large number of alternatives showed to be very pronounced, resulting in significantly larger S-scores in condition X than in condition III (Wilcoxon; $z = 5,22$ $P << .01$). This result, which was expected, means that adding a task tends to result in

more random sequences, the effect increases with the number of alternatives.

18. It was hypothesized that the effect of a task on the number of repetitions would be the reverse, no effect in the case of a large number of alternatives, but an appreciable one in the case of a small number of alternatives. Both predictions were realised. Comparing the number of repetitions in condition X versus condition III (9 alternatives) yielded no difference (Wilcoxon; $z = 1.14$) but:

19. A difference was found when comparing the number of repetitions in condition V versus I (Wilcoxon; $z = 4,53$; $P << .01$), the number of repetitions being less in condition V. Introducing a second task increases the effect of the tendency to avoid repetitions, and the effect is stronger the fewer the number of alternatives.

20. The instruction not to care about equal chances for all alternatives, showed a marked effect on the S-score values as well as the number of repetitions indeed; Compared with the immediate previous choice task, in which the same pattern of 5 alternatives was applied, (condition X), the instruction (condition XIV) showed to increase the S-scores (Wilcoxon; $z = 4,60$; $P << .01$) and the number of repetitions (Wilcoxon; $z = 3,58$; $P << .01$). This means that an instruction requesting the Ss to choose non randomly actually leads to more random sequences, which supports the assumption, that the Ss concept about randomness plays an important role. The finding that the number of repetitions increased also supports the assumption that the tendency to avoid repetitions is a consequence of the S concept of equal frequencies for each alternative.

21. It was hypothesized that no zero order effect would appear when all alternatives are in a relatively equal position with respect to each other. The arrangement of the alternatives in the conditions I, II and III fulfil this requirement. The data about the zero order effect in these conditions are given in table 11.

Table 11: Sum of ranks for the alternatives in condition I, II and III.

Condition I	a=2	Chi^2=	df=1	n.s.	
alternative	1	2			
S_{ranks}	93	84			
Condition II	Chi^2= 2,0	df=4	n.s.		
alternative	1	2	3	4	5
S_{ranks}	180.5	184	164	184	172.5
Condition III	Chi^2=		df= 8	P < .01	
alternative	1	2	3	4	5
S_{ranks}	275.5	368	287	256.5	233.5
Condition III					
alternative	6	7	8	9	
S_{ranks}	312.5	308.5	311.5	302	

The data of the conditions I and II confirms the hypothesis. However, the data of condition III are contradictory. Therefore it is probably safer to reformulate this hypothesis and to put it in the following words: If all alternatives are within equal reach, the lowest zero order effects will be found when all alternatives are given a relatively equal position towards each other. The data of I and II, and of the conditions IV, VI and VIII compared with II, support this hypothesis.

CONCLUSION

Four out of the 21 hypotheses tested were not confirmed by the results of this experiment. The effect of a difference in attainability of the alternatives could not be shown, but this might be ascribed to the fact that in this condition there was no difference in attainability at all. That no difference was found in the amount of recall between the position or content of the previous choices is disappointing,

but not ver damaging to the theory.
More serious are the findings that with 9 alternatives arranged in a circle still zero order effects appeared and that with 9 alternatives information did not effect the tendency to balance the frequencies of the alternatives (although the latter result must be partly due to the already extreme low S-score values found when no information was offered).
Therefore the general conclusion can be, that the theory stand the test rather well and becomes confirmed in various aspects by the results of this experiment, but that carefulness is required in the case of a large (>9) number of alternatives.

ABSTRACT

Based on data of ESP experiments it appeared that generating sequences of choices from a limited number of visually presented symbols mainly yields response preferences which can be classified into zero order effects, a tendency to avoid repetitions and a tendency to balance the frequencies of the symbols. A zero order effect means that in spite of the tendency to balance the frequencies of the symbols a significant difference between these frequencies is found. Similar phenomena appeared in investigations in which Ss are explicitly asked to generate random choices. A theory is constructed in which these data are explained starting from the assumption that Ss put on a level the concept of equal chances for all symbols with the concept of equal frequencies for all symbols. In addition the concept of randomness is associated with the concept of being non-systematic. Ss try to equalize the frequencies by selecting successively small subsequences based mainly on the position of the symbols. They avoid being systematic by not repeating the same subsequences. Recall of previous choices does not play an important role in this strategy. From this theory a number of predictions can be made, which together with hypotheses related to the data and the assumptions on which the theory is based, were tested in the present-experiment. Only four out of the 21 hypotheses were not confirmed by the data of this experiment. Therefore it can be concluded that the

theory stands the test rather well.

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