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DRAFT
June 1976

UPON THE REPLICATION OF SCIENTIFIC FINDINGS: A DISCUSSION

ILLUMINATED BY THE EXPERIENCES OF RESEARCHERS INTO

PARAPSYCHOLOGY

To be presented at the 4S/ISA First International Conference
on Social Studies of Science.

Cornell University

November 1976

1. Introduction. Situation of the Paper in the Context of the Sociology of Science

One reading of Kuhn (1962) encourages the sociological researcher to treat scientific findings as epistemologically equivalent to any other cultural artefact. Within this tradition, it is reasonable to try to show the influence of political and cultural milieux upon the content of ratified scientific knowledge.¹ Nevertheless, the most widespread view is that social and political factors may cause only temporary aberrations in the progress of science or, at most, the re-direction of scientific attention one way rather than another. To most, scientific knowledge which has been established seems intractably certain - what has been discovered is the only possibility irrespective of the circumstances.

If the strong programme of the sociology of scientific knowledge (Bloor 1973) is to have conviction it must be shown that the findings of even the hard sciences are tractable. The first contribution that this paper is intended to make to this programme is to render less crystalline the findings of science by closely examining and revealing the social component of one important aspect of scientific method. It is intended to help push out the boundaries of relativism,^(Collins + Con 1976) and to help show the reader how it is possible for one result rather than another to be 'discovered' in the day to day work of scientific laboratories.

The aspect of scientific method to be looked at is the replication of experiments.

Belief in replicability is of constitutively vital importance in science. Replicability, can be seen as the operational equivalent of natural regularity, or to put it another way the institutional equivalent of inductive generalisation. To use a phrase borrowed from Pollner (1974) it is one of the most important modes of 'accomplishing objectivity' in scientific practice. This importance is reflected in the theoretical and practical adherence to the criterion of replicability by those in less well established fields who wish to claim scientific legitimacy,² and in the claim of 'replicability in principle', by any 'scientist on the Clapham omnibus' who one cares to press for the reasons for his belief in any phenomenon of science which he has not personally verified.

In formal terms, Karl Popper has stated:

"We do not take even our own observations quite seriously, or accept them as scientific observations, until we have repeated and tested them. Only by such repetitions can we convince ourselves that we are not dealing with a mere isolated 'coincidence' but with events which, on account of their regularity and reproducibility, are in principle inter-subjectively testable". (1968 45)

and again

"any empirical scientific statement can be presented (by describing experimental arrangements etc.) in such a way that anyone who has learned the relevant technique can test it". (1968 p. 99)

and Werner Heisenberg:

"The essential basis for its /physics/ success was the possibility of repeating the experiments. We can finally agree about their results because we have learned that experiments carried out under precisely the same conditions do actually lead to the same results".

(1975 p. 55)

By unpacking the process of replication in detail, I hope to reveal the extent of the tacit understandings which underlie the claims made above, and show that these confident statements mask a complex of argument over their applicability.

To see the process of replication operating in detail, I have chosen to look at a controversial area of the pursuit of knowledge - ^{2a} parapsychology. This field has recently been defined as:

"...the scientific study of the 'paranormal', that is, of phenomena which in one or more respects conflict with accepted scientific opinion as to what is physically possible". (Beloff 1974 p. 1)

(my stress)

The potential and actual conflict of paranormal findings with science, forces many criticisms of the field, and defences of orthodoxy into the open. In a sense scientific parapsychology starts with familiar scientific scenes and procedures and then 'makes trouble' with its strange findings. (Garfinkel 1967 p. 37). The quest for the 'mistakes' that 'must have been made' in parapsychological research has involved lengthy discussion of orthodox scientific protocol in the attempt to demonstrate deficiencies in parapsychology's procedures. As a result, various normally 'taken-for-granted' procedures such as replication, have been analysed in some detail, and their less formal aspects have been revealed. At the same time parapsychologists themselves have been engaged in similarly revealing quests for the 'touchstone' of scientific legitimacy. Thus, while the demonstration of the tractability of parapsychological findings which follows might be dismissable as 'a special case', features of replication in science in general are brought out in the confrontation with the orthodox. Examples of such arguments may be found in figures 1 and 2.

FIGURE 1

1. From Beloff (1970 p. 131/2)

One rule that science insists upon is that no new discovery be admitted to the general body of scientific knowledge until it has been checked and corroborated by some independent investigator. Only thus can we insure against the fallibility of the individual worker. In parapsychology, however, although we pay lip service to the principle of repeatability, we indulge in every manner of special pleading in order to cover up our failure to honour it...if we want to be treated on a par with the other sciences, then we cannot flout the very criteria that are used to distinguish the genuine from pseudo-science. p. 131/132.

2. From Crumbaugh (1966 p. 526)

...the only criticism not yet adequately met by the present data, and the one which I contend is crucial and must be met before the great bulk of scientists will swing over to acceptance of the ESP hypothesis... (is)...the failure of ESP experimenters to produce a truly repeatable experiment - one which can be replicated in almost any laboratory as many times as desired with essentially the same results. Repeatability has long been cornerstones among the requirements of sound methodology in all experimental science.

3. From West (1956 p. 17)

(the best experiments in parapsychology) fall short of the requirements for universal scientific conviction for several reasons, the chief one being that they are more in the nature of demonstrations than repeatable experiments...No demonstration, however well done, can take the place of an experiment that can be repeated by anyone who cares to make the effort.

4. From Nicol (1956 p. 28)

...repeatable experimentation /means/ ...the designing of an experimental set up which, found in practice to produce a significant effect, can be repeated by any competent person at any time in the foreseeable future with approximately similar significant results. After thirty years, psychical researchers have failed to produce one repeatable experiment.

Quotation from 'Discussion'
in Wolstenholme & Millar (1956)

FIGURE 2

Gaddum: (Pharmacological Laboratory Edinburgh University)

One kind of evidence that I feel is really convincing is some people can do it and some cannot. I mean, if it was the sort of thing that anybody might do sometimes, I should not believe in it so readily as in the fact that once you have got a man like Shackleton, you can apparently make repeatable experiments with him. I do not understand why it is suggested that it is not a repeatable experiment, if day after day you can always get significant scores. (My stress) p. 39

Pratt: (Parapsychology Lab. Duke University)

I cannot see how there is any serious question to be raised regarding the repeatability of an experiment when Dr Soal and Mrs Goldney, for example, working with their selected subjects week after week, were able to get results and were able to do so with visitors brought in to witness and even to take charge of their experiments...to his way of thinking he had successfully repeated the Huko work. The thing to keep in mind, it seems to me, is the essential nature or the essential feature of the experiment, and if similar results are obtained by a number of people working in different places, then I think that in every real meaning of the term the results are repeatable. (Pratt's stress) p. 40.

Wasserman: (Dept. of Mathematics, Kings College, Durham)

....I do not think we can use the word repeatable without caution. ...there are two types of repeatability... repeatability within a single experiment...and between different experiments, and these two types of repeatability are entirely different things.

Let me also discuss...repeatability at will...Looking for such a type of repeatability is looking for a Mare's nest. Take for instance experiments in cosmic physics. Sometimes you find a meson track, sometimes you do not. You can set out with iron determination to find a track and come home empty handed. Nevertheless, physicists agree that these tracks even if rarely found are important. In other words, if we have a rare event we cannot expect it to be repeatable at will. We must simply distinguish between high and low inductive probabilities, and this I think Mr Fraser Nicol does not seem to appreciate.

(Wasserman's stress) p. 41

J. Langdon-Davies (origin given as Girona Spain)

It seems to me...rather interesting that the para-psychologists all fall over backwards and say "We must have repeatability", but that the biologists at this symposium say that this does not impress them as a primary necessity. p. 42

Spencer-Brown: (Christ Church, Oxford. Critic of Parapsychology)

As someone pointed out, we can get repeatability within a single experiment, but this is not what is meant by repeatability in science. We want results which are not only consistent in one experiment, but which can be observed to recur in further experiments. This does not necessarily mean that they must be repeatable at will.

A total eclipse of the sun is not repeatable at will; nevertheless it is demonstrably repeatable - we can give the recipe for its repetition. And this is the minimum we look for in science. We must be able to give a recipe. (My stress) p. 44)

West: (Society for Psychical Research, London)

....some experimenters may be incapable of repeating the results which other experimenters are able to obtain. That is what I meant by factors of repeatability. (p. 45)

Nicol: (Cambridge, Massachusetts)

....Dr Wasserman is clearly giving the word a meaning and definition that are not usually attached to it in scientific thought. Dr West apparently, and certainly I, were unaware that the repeatability problem, which has defeated many years of effort, has in fact been solved and we did not notice it The Shackleton case etc/...do not constitute repetition as I understand it. I mean by repetition and I am rather under the impression that in the physical and other sciences this is also the understanding - that you design an experiment which any competent person can repeat with approximately the same results, and now come the essential words, at any time in the foreseeable future.

If....you can get a result repeated over and over again, then you have only to describe it to any other competent person, psychologist, physicist, medical man or anyone else, invite him to do it for himself and he will get the same results. When you can do that successfully you will have won half the battle for recognition. (Nicol's stress) p. 48

2. The Schema for Analysis of Replication in Parapsychology, and Suggestions for Wider Application

To guide the analysis of the first part of this paper, I am going to examine the notion of replication in an unusual way. Instead of taking replication as an unproblematical notion, and searching out what might be called its 'sociological loopholes', I will proceed by asking how a series of replications (in parapsychology) might be recognised. In this way, a series of stages of demarcation are generated, which might be suggestive in the analysis of areas of science not dealt with here.

Thus: imagine there has been some purportedly successful parapsychological experiment E performed by scientist S at time t. After time t the life of the world goes on, and is filled with multifarious activities. How can it be decided if some subset of those activities render E a 'successfully replicated experiment?' Imagine a search is to be conducted through all the post-t activities in order to find a set of replications of E and make the appropriate decision, and suppose that the procedure adopted is to remove from consideration all activities that could not count as replications of E for one reason or another, so that in the end all that is left is a residue which consists of the appropriate set. A series of decisions would be required such as the following:

Stage 1 Reject all activities that are not to do with the phenomenon of parapsychology.

Stage 2 From the remaining set of activities, reject all those which aren't experiments.

Stage 3 From the set of experiments, reject all those where the identity of the experimenter is inappropriate.

Stage 4 From what remains, reject all experiments that were not competent copies of E.

Stage 5 Divide the remaining set into those which generated negative and those which generated positive results.

Stage 6 Decide whether E has been replicated.

The working of this schema in the particular case of parapsychology will be examined below, and it will be seen, that all of the six stages of demarcation are open to ambiguity in application. That this is the case, is indicative, I believe, of the current state of scientific parapsychology. It demonstrates the undeveloped unconstraining unconsensual nature of parapsychological culture. This lack of constraint which might well have been suspected on casual acquaintance with the field can thus be made evident in ^{an examination of} the actual practice of experimental work. I believe that comparative analysis of a similar type, in other fields of science might be illuminating, and might be suggestive of the way in which experimental areas are embedded in the wider culture of science, and of the way they develop. In other fields, none or only some of these stages may reveal ambiguity. For instance, in the area of detection of 'gravitational radiation' there is no difficulty in assessing which activities are concerned with the phenomena of gravity waves, nor too much problem about deciding what constitutes an experiment, but all the other stages present difficulty. This suggests a slightly higher degree of embedding and cultural closure. On the other hand, in another area investigated by the author - laser building and testing - no stage presented any difficulty in assessment. ^(Collins 1974, Collins + Harrison 1975) Indeed, that area of experimental activity exemplified an interesting characteristic of the schema, (and a suggestion that it may have certain properties of transitivity) namely that disagreement at all the earlier stages is impossible once agreement has been reached at stage 6, for all successful replications then become completely identified by their production of results of an acceptable sort. The list of stages then collapses and what is referred to below as 'the sociological solution to the problem

3. The Demarcation of Replication in Parapsychology

Stage 1

There is difficulty in demarcating those activities which have to do with parapsychology and those which do not, because so little is known about the way psi-phenomena manifest themselves. (If they do so). For instance, one theory holds ^{that} the fundamental biological structure of all living things is organised at the cellular level through paranormal channels of communication. If this were accepted then, for example, every case of differentiation of a blastocyst³, would be an evident case of psi-manifestation. (There is so far as I understand no 'orthodox' explanation available anyway). Thus it may be construed that embryologists regularly observe the functioning of psi. Setting this aside, it will be seen that inability to determine when psi effects are present is responsible for indeterminacy in many of the stages which follow.

Stage 2 ^{3a}

FOOTNOTE
3a { [Demarcation at this stage will eliminate most 'spontaneous' and anecdotal evidence which in the case of psi, plays a large part in forming and reinforcing belief in the phenomena, and in many cases will be the motivating force in long boring and often disappointing series of experiments. That this is so, suggests that replication is a legitimating device, or a device to persuade others, not a test for a phenomenon for most scientists in this area.]

Demarcation at this stage/distinguished, from, for instance, a preliminary observation. Critics of parapsychology have been quick to point out that for the field as a whole, the aggregate statistical confidence, in the aggregate number of experiments conducted to date, may be offset by the much larger number of experiments which have obtained negative results.⁴ In other words, in the context of a large field of negative findings, the positive results may be no more than the expected tail of a normal distribution. In part the validity of these arguments

depends on the demarcation between experiment and 'observation' being made in such a way as to make the statistics 'come out' in favour of one side or the other.

The same argument may be used regarding a series of replications of one experiment. The argument need not necessarily rest on a statistical base. For instance it may be argued that a claimed negative replication, was 'really' a preliminary observation. (see below p. where such an argument is made). Also an experimenter's classification of his own work might change as his knowledge of the field increases. (see below p. ⁵)

It seems then that decisions at stage 2, which will affect the balance at stage 5 and the decision at stage 6, must rest on criteria generated outside of the experimental situation itself.

Stage 3

One sort of 'inappropriateness' is experimental incompetence of one kind or another, but consideration of this will be left to stage 4. Otherwise, experimenters might be 'inappropriate' because of the relationship they have with the original experimenter S. Thus one might look at the relative value of experiments performed by students of S, members of the same group, known sympathisers, known sceptics and so on. It might be hypothesised that the greater the social and cognitive separation of S and a repeating scientist, the greater the value of a positive replication, and vice-versa. This seems to be the case in parapsychology.

Concerning the special case, where the relationship of the replicating scientist to S is one of identity, one may note that though in parapsychology, critics often deny the validity of an experiment because the original experimenter did not repeat it, experimenters, in reply to this charge point out that they do not consider further work by themselves to be of value because it is also generally true that critics do not count a series of positive results by the same experimenter

make this point strongly. (Beloff a believer in the phenomena himself, but a much respected psychologist by profession, is here putting forward the ⁷ 'scientists' view).

On the other hand, a case for counting a long experiment by one group, as the equivalent of a series of replications, can be made (see Pratt fig. 2 and also Wasserman) but the comments of Spencer-Brown and Nicol in this connection represent a widespread view regarding parapsychology. An interesting extreme case was put to me by one famous critic of the field, who argued that he would accept no psi-finding until he had repeated the experiment himself, a situation which would surely be disastrous for most of the sciences.

Stage 4

This stage involves the most complex web of arbitration. In the first place it may be suggested that a phenomenon is best demonstrated by a multiplicity of experiments exhibiting different designs, because then the phenomenon cannot be attributed to an inadequacy in the original protocol which is repeated in the confirming experiments. As it happens many believing parapsychologists think that the existence of psi phenomena has been demonstrated beyond doubt precisely because it has been shown to operate in a variety of experiments of widely differing design, but ironically critics still demand the single replicable experiment as proof of the existence of psi.

For this argument to be settled, for instance, if all positive psi-experiments be taken as tests of some archetypal experiment, certain 'prior-to-test' assumptions must be agreed. Thus if telepathy, clairvoyance, and psychokinesis are admitted to be different manifestations of some deeper phenomenon then it could be argued that a collection of all psi experiments might make sense, but if these are different phenomena, then it would not.

What is certain, is that scientists who claim to find effects will not accept negative results arising out of experiments which were not 'identical' to their own. Non identity may be claimed by questioning the 'competence' of secondary experimenters or pointing out technical differences between a primary experiment and the others. The operation of this argument should become very clear in the case study material which (because of its length, and its bearing on another argument), will be discussed later. In the meantime, one may note points of relevance in figure 2. For instance Pratt's comments regarding the essential nature of the experiment, Spencer-Brown's comments regarding the necessity of a 'recipe', West's comments regarding the abilities of different experimenters and Nicol's remarkably wide listing of the class of 'competent person, psychologist, physicist, medical man, or anyone else', who must be able to repeat parapsychological experiments.

Stage 5

In parapsychology, it is not always clear whether an experiment produces a negative or a positive result. It is not possible to assess the relative size of classes of negative and positive experiments, because different commentators read experiments in different ways. For instance, one well known believer in the phenomena has - on his own account - consistently failed to get any positive results over a period of years. Often this failure has been explained away by others as due to some unspecifiable mode of incompetence (and this type of explanation adds to the problems of criterion 4) but others have suggested that this consistent failure is an interesting phenomenon in its own right demonstrating in a positive sense, one of the properties of paranormal phenomenon viz. a powerful experimenter effect. Hence, in some sense, this consistent series of failures can be reinterpreted as a positive result.

Other ways in which experiments may be reinterpreted as positive or null depend on the acceptability of certain hypothesised properties of psi phenomena, (and on the way results are grouped together) and on statistical techniques used in their analysis. Among the hypothesised properties of psi phenomena, is 'negative psi'. Thus, in some experiment where a subject is to make a series of guesses at the order of say, some randomised series, such as a shuffled pack of cards, a straightforward positive result, would consist of a score of 'hits' (correct guesses) at some statistically significant level above chance expectation. However, a not infrequent result in such experiments, is to find a score of hits which deviates significantly below chance expectation. This becomes a positive result provided the notion of negative psi be accepted, for it demonstrates positively the existence of a psi phenomenon, negative psi. Now, if one considers the way in which results are grouped together, one can see, that the aggregate result of two highly positive psi experiments - one demonstrating positive psi, and one negative - may be exactly chance - a null or negative result. More obviously, it is possible to generate overall positive results by stopping and starting experiments - or grouping them as good experiments, faulty experiments, preliminary runs etc. in appropriate ways. Parapsychologists nowadays run their experiments according to a protocol laid down in advance, so that crude post facto manipulations should not occur, but nevertheless, there is plenty of scope for the critic or other commentator to arrange experiments, so that they support his case.

Finally, there is the straightforward question of the level of statistical significance which is taken as satisfactorily demonstrating the phenomenon. The convention in parapsychology seems to be slightly more stringent than that in sociology. Parapsychologists prefer the 1%

level to the 5% level, but, it is interesting that - and this seems nothing more than convention - the levels of significance demanded in physics are far higher.⁹ On the other hand, some parapsychologists run their experiments for a sufficiently long time to produce astronomical levels of confidence (see footnote 4 and description of Schmidt experiment below), but critics do not seem to find these results particularly impressive.

Stage 6

Suppose then, a list of experiments, construed as competent copies of E, performed by scientists other than S, could be drawn up, and each have unambiguously attached to it, the ascription, positive or negative replication, what calculation would be required to determine whether or not the original experiment had been successfully replicated? There are three possible cases to consider; all the reported copies might be positive, all might be negative, or there might be some of each.

It is tempting to imagine that either of the first two possibilities would ensure an end to any doubts as to the existence of the phenomena in question, but this is not so. It is still possible to argue about the required length of a series for it to be taken as definitive. In the case of either negative or positive series, it must always be possible to ask that more experiments be done, to 'make absolutely certain'.¹⁰ In addition, note the comments by Gaddum, fig. 2, who seems to think that the very fact that not all results are positive is a further reason for accepting a phenomenon, and also note the point made above about the re-interpretation of a consistent series of failures as a positive result. Also, several commentators do not consider a series of positive results a very likely outcome - so much so, that results which are too good might perhaps be construed as demonstrating cheating - for they do not expect an experiment to be repeatable at will, (see the comments by Wasserman and Spencer-Brown.)ⁿ

Finally consider the case where some replications are positive, and some negative. What is the ratio of positive to negative experiments which gives confirmation to a phenomenon? The point has been

laboured enough - the reader may imagine the difficulty of setting up such a criterion.¹² Perhaps this short analytic exercise may be seen as a reductio ad absurdum of the notion of replicability put forward by Popper, and quoted above, and the most appropriate conclusion is that scientists do not actually make up their minds about the existence of a phenomenon by referring to a list of experiments which might or might not prove it but use the idea of replicability as a legitimating device. Of the comments in the figure, Langdon-Davies' may be apposite, in that the results of most biological science are not controversial. It would be of interest to see how the six stages outlined above would apply in other sciences, controversial or otherwise.

I will now examine two case studies of replication in parapsychology to illuminate further the points made above, ^{and} especially the social processes involved in demarcating experiments along the lines required for criterion 4.

4. Case study material

The source of this material, is published and unpublished reports of experiments, interviews conducted by myself, in Britain and the U. S. A. and some correspondence (with Professor Hansel). I have looked at arguments surrounding the replicability of two experiments in parapsychology. The first of these I have looked at in some depth. It is concerned with the emotional responses of plants. The second I have looked at briefly as it illustrates a point not brought out in the first case.

(A) Responses of Plants to Remote Stimuli

Basically, this experiment involves monitoring the changes in resistance of plant leaves, while the plants are subject to emotional stimuli. The apparatus required, is a sensitive Wheatstone Bridge type circuit which can detect changes in leaf resistance, an electrode system to link the plant leaf into the Bridge, and some means of providing the emotional stimuli. Fairly full but uncritical accounts of this work may be found in the first three chapters of The Secret Life of Plants (Tomkins and Bird 1974).

The first formal presentation of results is to be found in International Journal of Parapsychology (Backster 1968) entitled 'Evidence of a Primary Perception in Plant Life'. The author, Cleve Backster reports that an automatic apparatus was arranged to drop live brine shrimps at random intervals into hot water thus killing them. During the course of the experiment chart recorders monitored the electrical resistance of the leaves of three living plants. Backster claims that a statistically significant correlation was found between brine shrimp

to be found on control runs, where the shrimp were absent.

My fieldwork included interviews with American scientists, who had an interest in this work, or who had performed experiments themselves. A first round of interviews was conducted in 1972 and included Backster, and two others, who had claimed to find positive results in similar experiments. I was able to discuss with them, the results of a negative experiment, that they were familiar with. I wrote up an account of this field trip (for private reference), before setting out on a second trip in 1975.

Before setting out on the first American field trip, I attempted to repeat the Backster experiment myself. With the help of colleagues from the Physics department of the University of Bath several plants in turn were linked to a Wheatstone Bridge and chart recorder via aluminium electrodes, while stimuli such as threats, lighted matches brought near, etc. were administered. Our chart recorder showed large fluctuations in leaf resistance but none of these could be correlated with any of the stimuli. Our conclusions were pessimistic. I mention this, for two reasons. Firstly, to indicate the level of detailed technical proficiency which had been acquired before setting out in the field. Secondly, because it was a good example of work requiring reinterpretation. After speaking to Backster for only a short time, it became clear that this 'experiment' should be classed as only the most preliminary observation.

The 1972 field trip

A short account of the negative experiment discussed in 1972 is to be found in The Journal of Parapsychology (Johnson 1972) in the form of a letter from R. V. Johnson. Johnson wired up his plant in the way described by Backster, and discovered similar fluctuations in leaf resistance, but without any correlation with emotional stimuli. Upon putting his plant in a controlled environment, the unexplained fluctuations

degrees centigrade, or the humidity by 10% - 15%. Johnson claims competence for his experiment at the outset by citing the authority of orthodox scientists. Thus he writes:

"I did my research under the careful scrutiny of the electrical engineering and botany departments at the University of Washington and received my Master's degree based on my work".

All three of my respondents knew the Johnson experiment quite well, and I asked them how they coped with what seemed to be watertight competent criticism of their results. As it turns out, they saw this as no particular problem, and indeed were surprised that the Johnson work had received the attention it had. The following is a list of replies from the three. As this is interview material, I have not noted the origin of individual remarks, and have not grouped them according to their origin.

First, some general comments concerning Johnson's experimental competence:

- A. "...Most people when they don't follow the instructions exactly have the sense to keep quiet about it..."
- B. "... (the original papers) are very misleading because they give the impression that just anyone can go out and do it, and you can't.
- C. What he has done is to describe the experiment as unmanageable in his preliminary observation, which doesn't give him a voice say anything".

Next, some detailed criticisms of Johnson's techniques - (Note that these are the remarks of three different respondents, so they are not necessarily in agreement.)

- D. "In changing from an AC to a DC bridge he lost the phenomenon completely".
- E. "...Johnson's results were quite correct but the difference is that in my experiments I'm interested in very short term responses, for instance responses that take place in less than five seconds, so that the long term responses reported by Johnson do not really count."

- P. "Johnson's results were nonsensical because he used aluminium foil and you must use one of the noble metals for contacts in order to avoid the oxidation effect, which will create artifacts from now until the aluminium foil completely corrodes away ... the effects that Johnson was measuring, the effect of humidity, CO₂ concentration, and so on, were so large as to mask the effects of the oxidation of the aluminium foil so that he got good results for what he was after. But this is nothing like sensitive enough to find the Backster effect. It's no surprise he didn't find it".
- G. "He didn't isolate the plants for a period before the experiment".
- H. "...it's completely haywire, and in fact what you must first do is make some sort of energetic connection with the plant and then you can hold the polygraph /resistivity measurement device/ completely steady and it is no longer affected by temperature changes. - thats after you've made a sort of p~~h~~ysical connection with the plant. Developing a psychic link between yourself and the plant is like suddenly dropping a Faraday cage round it.."

The Johnson experiment was then, interpreted differently by different observers. Though in 1972, it was seen by the many critics of the Backster findings as a definitive refutation, being good enough to merit a report in the Journal of Parapsychology, believers in the effect, were able to dismiss it out of hand on the grounds of incompetence, or technical deficiency.

The 1975 Field Trip

Between 1972 and 1975 the results of two other negative experiments were widely publicised.¹⁵ My sources in 1975 consisted of interviews with Backster, with these two new critics - Drs. Gasteiger and Kmetz, - discussions with other interested parapsychologists, various documents including Heronitzel (1975) and tape recordings of a symposium held at the American

Association for the Advancement of Science (AAAS) annual meeting in January 1975, where among others, Backster, Gasteiger and Kmetz presented papers and answered questions.

A significant difference between the first and second field trips, was the almost total absence of discussion of the Johnson work in 1975. It seemed that now, critics and believers alike, agreed that the Johnson experiments were not sufficiently technically competent to count as refuting instances of Backster's results. This may have been a consequence of the general increase of technical knowledge in the field. Advances in what counts as 'expertise' certainly did take place in the meantime for both negative experiments were conducted in an atmosphere of continuing consultation with Backster himself. Equipment was loaned to Kmetz by Backster, and Gasteiger's graduate students met Backster to discuss the experiment with him. There is no reason to suppose that either group set out with specifically negative intentions; rather; they appear to have taken the experiment very seriously. Indeed Gasteiger, when pressed, was prepared to concede that the residual differences between his own and Backster's experiment could conceivably account for the differences in their results, but as he put it, there is a point at which one can no longer continue to make further adjustments to one's experiments but must publish what has been found to date - negative results. But, both these experimenters developed their expertise in the work far beyond Johnson's. My temerity in making this 'professional' judgement is based on Johnson's own statements that his initial intentions had been to

'try to obtain Backster's results independent of his recommendations' (Johnson 1971 p.3) and that 'After completion of the experiments silver foil was recommended as a better electrode material than aluminium. No experiments were attempted with silver foil though as it was not considered important enough to change the results.'

Perhaps then, the more recent lack of discussion of Johnson's results is a consequence of the 'public education' following the more recent publications, or the AAAS meetings, but a functional explanation is tempting and reasonable. It seems likely that with the new 'definitive' refutations performed by experts, critics no longer 'need' the Johnson result, and can now afford to admit to its technical limitations. The AAAS symposium presentations may now fulfil the function of removing Backster's results from serious consideration.

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Neither of the more recent negative experiments affected Backster's confidence in his results. This was made clear in my interview, and in his short response to the critical experiments at the AAAS. The latter began as follows:-

...I feel as though its after the crucifixion, but lets make believe its Easter, and lets see if I can rise now...My understanding of scientific methodology is that before you can actually fail to repeat an experiment, you must attempt to repeat it. And when you attempt to repeat ^{it,} you must precisely follow the direction of the original experimenter./but/ I have heard time and time again of the adjustments they made because they thought it was better or easier. This is not replication in any sense of the word.....There has been no successful failure to replicate that experiment.....

Backster then listed the technical deficiencies of the negative experiments, as follows: (My paraphrase)

1. One experiment was performed by graduate students, and as they work to deadlines, and therefore under pressure, it is probable that they were not psychologically attuned to obtaining results in the area of consciousness research.

2. The experiments were carried out during the day, in the normal environs of a biological laboratory, and as it seems that stimuli might be 'received' from any part of the building in which the plants were kept, and as there was likely to be a great deal of biological 'noise' in the building, the plants might be expected to show no outstanding reaction to the experimental stimuli.
3. More brine shrimp 'drops' were used in each run of the reported experiments than in the original experiment. As the plants show an 'adaption effect' this 'dilutes' the data unnecessarily, perhaps to a statistically insignificant level.
4. Switching from a D.C. to an A.C. bridge might destroy the effect.

...We don't even know what this phenomenon is. We haven't even identified it. If there is a remote effect - if there is an attunement to consciousness...how can we arbitrarily jump from one technique to another ... (verbatim)
5. Relationships between experimenter and plants were not carefully controlled in the appropriate manner before the experiments.
6. The monitoring equipment was used on automatic recentring mode which it had been stressed, disturbs this experiment for unknown reasons.

It seems both from the tone of the conference, and from his own comments, and others' reports, that Backster was not finally crucified by the AAAS symposium.

B. The Schmidt Experiment

The second case shows that experimenters who accept a phenomenon may get negative results from their experiments yet not claim that this demonstrates the non-existence of a phenomenon under investigation. Here subsidiary explanations are used to explain, not why someone else's experiment did not work, but why their own did not. That is, to use the term in a broad sense, why their own experiment was not a competent replication. On the other hand, the remarks of a critic demonstrate that he fills out the notion of competence in precisely the opposite sense to the believers in the phenomenon.

This second example differs from the first in being what all parapsychologists (so far as I know) would count as an exemplary experiment of its type. Indeed, the New Scientist a popular but 'straight' British science magazine published an account of the experiment and included an editorial which presented it as an answer to one of parapsychology's most trenchant modern critics C.E.M. Hansel. Thus:

Not long ago in "ESP: A Scientific Evaluation" (MacGibbon and Kee 1966 p. 241) Professor C.E.M. Hansel concluded a critical survey of parapsychology with the words: "If 12 months work on VERITAC (a machine used by the US Airforce Research Laboratories) can establish the existence of ESP the past research will not have been in vain..." Dr. Schmidt's machine seems in no way inferior to VERITAC and he has now come up with positive results. It remains only for other investigators to confirm these findings. (16 Oct. 1969 p. 107)

Schmidt, a physicist working for Boeing, reported the construction of a machine in which four circuits linked to a four lamp display were opened or closed at random as a function of the decay of a quantity of Strontium 90. (Schmidt 1969 a, b, c). Subjects could complete the circuits to the lamps by pressing one of four buttons placed beneath them. One of the four lamps would then light, the particular one being determined

by the position of the radioactive decay controlled switch. Subjects would aim to 'guess' the position of this switch by pressing the button beneath the lamp they expected to be next to light. They should succeed one time in four on the basis of chance expectation. Details of the device itself, and details of tests for randomness of the output when the buttons were pressed automatically were published in a six page article in the Journal of Applied Physics (Schmidt 1970) a high prestige orthodox physics journal. In the New Scientist, and the other publications mentioned, Schmidt gives details of the performance of certain subjects, attempting to press the appropriate buttons (hits and misses were recorded automatically). Over 20,000 trials, four subjects between them scored at about 8% away from chance expectation, a score which should occur only one time in 10^{10} (one in 10 thousand million) if due to chance. Schmidt attributed their success to some psychic effect.

At the time of my fieldwork (in 1972) several positive replications had been informally reported, but, for the purposes of this paper the interesting result was the negative replication reported by Beloff and Bate (1971), titled 'An Attempt to replicate the Schmidt Findings'. The authors introduce their report with a description of Schmidt's results claiming that

...not only are they of historic importance but constitute one of the most rigorous demonstrations of an ESP effect in the whole parapsychological literature (p. 22)

and referring to the challenge set out by Hansel and referred to above (p. 20)

On Hansel's own showing, therefore, it seems we must conclude that ESP has now been established and past research has not been in vain. (p. 23n. Beloff and Bate's stress)

However, after an elaborate test of 18,650 trials with five subjects and a complex analysis searching for any extra-chance effects they were forced to conclude that:

Clearly the attempt to replicate the Schmidt findings has failed.

(p. 30)

Far from questioning the existence of the effects demonstrated by Dr Schmidt however, the authors put the following gloss on their findings:

We want to make it clear that our own failure to get positive results in no way detracts from Dr Schmidt's success. Why he should succeed and we fail must remain a matter for conjecture.

We think it most unlikely that the explanation lies in the difference between our different machines. Nor can it be attributed to the greater length of the Schmidt series since his good subjects show an upward trend almost from the beginning. There remain the imponderables of Dr Schmidt's personal magnetism, his manner authority etc. that may have inspired his subjects to greater efforts. All we know for the time being is that he found subjects who gave him significant scores, we did not. (p. 30)

Beloff and Bate then, are prepared to extend the boundaries of legitimate subsidiary explanation to the personalities of the experimenters, or to unspecified qualities of the subjects, in order to save the phenomena.

The opposite side of the coin was presented in an interview and subsequent correspondence with Prof. C.E.M. Hansel who was asked how he responded to the claims that the Schmidt findings established ESP on his own criteria. Prof. Hansel had reservations regarding the Schmidt apparatus, because of its use of integrated circuits, which in general, need a great deal of debugging. This reservation was backed up by his unwillingness to accept the results of one experimenter.

"Scientists in general have never trusted investigators. Every result has to be confirmed". (Correspondence)

"Schmidt should have got other independent investigators to check his subjects to confirm his results. As it is Beloff has repeated his experiment using other subjects and not confirmed the result". (Correspondence)

In the case of the Schmidt experiment not all those who have repeated it have got the same result. If half of them get the same result, and half of them don't one might think that the competence of the investigator was an important variable. One tends to discover that as soon as one applies rigorous methods in ESP experiments the results disappear. (My stress, my paraphrase of interview material with Prof C.E.M. Hansel).

As can be seen the Schmidt findings present no special problem for the critic of paranormal phenomena, there are several ways in which they can be explained away. The stressed section in the above quotation is particularly interesting. At first glance it would seem that Hansel is making precisely the same point about the competence of the experimenter as is made by Beloff and Bate to explain away negative findings. But what Hansel means by a competent experimenter is explained in the following extract from correspondence.

"In a particular case, if half the investigators get a result and the other half fail to confirm it, either group could be incompetent or fraudulent. But when the one result is fully consistent with contemporary scientific ideas and the other radically contradictory to it, the investigators who fail to get a result are more likely to be competent and/or honest".

as Schmidt they are likely to be incompetent - insufficiently rigorous.

For Beloff and Bate (and most other parapsychologists) consistent failure to get results is taken as demonstrating some imponderable personality defect which renders the experimenter incompetent.

5. Discussion

A. Negotiating the Existence of Phenomena

The case study material, as well as illuminating various points in the earlier analysis of replication throws particular light on the use of the notion of competence in arguments over experimental replication. It provides evidence for the applicability of a 'two-by-two' classification of scientists' use of this notion put forward by the author in connection with research into gravitational radiation (Collins 1975). In the case of parapsychology scientists' attitudes to experiments can be broadly summarised as follows.

		<u>Believe in Phenomena Under Investigation</u>	
<u>Find</u>		<u>Yes</u>	<u>No</u>
<u>Results</u>	<u>Yes</u>	1. Competent	2. Not Competent
<u>Consonant</u>			
<u>With This</u>	<u>No</u>	3. Not Competent	4. Competent
<u>Belief</u>			

These categories will be found to fit the attitudes of actors discussed in the case studies above.

I would suggest that in fields where the existence of some phenomenon is universally accepted (or not) the right (or left) side of this classification, boxes 2. and 4. (1. and 3.) disappear, and competence is then defined by the results found in the experiment. One might say, that the problem of induction is then solved, for, rather than asking how one can be certain that nature will always continue to act in the same way in the same circumstances, the question is stood on

its head. Where nature appears not to act uniformly . it is said that the circumstances must have been different - e.g. an experiment was incompetently performed. One might refer to this as the sociological solution to the problem of induction. In a controversial field, actors may be seen as arguing for the acceptance of such a solution, arguing for either the right, or left hand side of the table, and claims for replicability (or otherwise) are part of the rhetoric of these negotiations.

B. Negotiating the Character of Phenomena

The outcome of these arguments settles not only the existence of phenomena, but also their character.¹⁹ The negotiations over what is to count as an experimental replication determine the content of future inductive generalisations. Looking at the case study material, I think that this process can be seen with great clarity.

Backster's experiments made use of standard scientific techniques in the attempt to demonstrate the existence of emotional responsiveness in plants. That is he used control runs, blind judging, automated machinery, statistical analysis and so on. Backster and others were able to criticise the failed replications by pointing to inadequacies in their protocol, but these claims of inadequacy are usefully separated into two types. On the one had their are technical inadequacies of a type which could be acceptable to, say, any plant physiologist, these being lettered B to F (A is an ambiguous case) in the discussion, (p. 16) and on the other hand, there are inadequacies which would not be acceptable in this way. These are responses G and H, and the replies made in 1975 to criticism at the AAAS, all of which demand special psychic conditions for the plant experiments to succeed.

Some further comments by researchers who claim positive results, may make this difference more clear. Thus one of my 1972 respondents, in explaining his success, as compared with the failure of others to get results agreed that his knowledge of biology was weak, but commented:

"I feel however that my background in the spiritual world, also my personal experiences with the thing that I'm working with, makes me far more qualified than most people who have Doctor's degrees in biology because I've found these people to be very rigid. I've found them to be very fixed in their area, and their training has actually seemed to be more of a restriction in this kind of research than a help.

And Marcel Vogel discussing the general problems of replicating the experiment, has made the following points in print (Tompkins and Bird 1974 p. 46).

"If they approach the experiment in a mechanistic way and don't enter into mutual communication with their plants and treat them as friends they will fail!

"...empathy between plant and human is the key... No amount of checking in laboratories is going to prove a thing until the experiments are done by properly trained observers. Spiritual development is indispensable. But this runs counter to the philosophy of many scientists who do not realise that creative experimentation means that the experimenters must become part of their experiments".

Finally, somewhat of the same shift in emphasis is evident in the second case study, where the researchers themselves refer to some unknown mode of incompetence to explain the failure of the Beloff-Bate experiment.

Now, if in selecting a set of experiments to constitute a successful series of replications, believers in psi phenomena, select one set as competent, and another as incompetent as a function of the type of factor mentioned immediately above, then in negotiating the acceptance of this set - in which project they may well succeed if they are sufficiently persuasive - they have not only negotiated the existence of the phenomenon, but have also negotiated its character - i.e. it is of such a nature as to be affected by certain subtle psychic influences, and not such a nature that the mere scientist, trained in a scientific orthodoxy could hope to discover it. In this way, can a new phenomenon come into existence, and thus can its characteristics be socially determined.

In the field of parapsychology in general, precisely such a process has taken place. The first four quotes in figure 1, represent the opinions of only one school of parapsychologists - perhaps a fading school. Others, and these include many of the brightest young parapsychologists, believe that the existence of psi phenomena has been more than adequately demonstrated in the hundreds of positive experiments that have been reported over the years. They think it a waste of time to continue to seek to prove the existence of psi to a sceptical world but now take its existence as proven, and try to learn more of its characteristics. But in making this decision they have already constructed some of its characteristics. For them, not only is psi a workaday phenomenon, but also a frequently reticent phenomenon. Thus they already 'know' something about it. And by seeing that, for instance, certain individuals never seem to be able to see it all, they 'know' more about it still.

6. Summary

The object of this paper has been to show 'how one result rather than another might be discovered in the day to day work of scientific laboratories'. A major obstacle in the way of the acceptance of such a 'tractability' in scientific findings, is the belief that genuine results evidence themselves by their repeatability, so that the criterion of replicability distinguishes the unique set of genuine results from the set of false ones. I have tried to show that this obstacle is less substantial than it seems, and indeed, to show that it is precisely in 'negotiations' over the replicability of phenomena that one result rather than another might be 'discovered'. On this reading, replicability should be seen as part of the 'rhetoric of scientific presentation' - a means of accomplishing objectivity, rather than demonstrating it.

- - THE END - -

*I am grateful to colleagues in the sociology group at Bath University for encouragement and helpful comments on drafts of the paper, and to colleagues in the physics department for help with the experiment. The fieldwork was paid for in the nicest possible way by the Social Science Research Council. My greatest^{left}/however is ~~due~~ to the scientists who I spoke to, all of whom responded with charm and friendliness.

1. The possibility of generating a sociological view of the knowledge of the hard sciences has been given credibility by Kuhn (1962), though a philosophical grounding for this viewpoint is to be found in Wittgenstein (1953, 1956, Winch 1958, Bloor 1973, see also Collins 1974, 1975). Feyerabend's methodological writings also are important in this context (1975), but the work which has had probably the greatest influence upon researchers in the field, even though its author would not have accepted the interpretation implied here (see Feyerabend 1975) is that of Lakatos (1963-4, 1970, Barnes and Law 1976). Lakatos' demonstration and discussion of the importance of the ad hoc hypothesis in debates surrounding the acceptance of mathematical theories and scientific discoveries has stimulated one of the major arguments of this paper. Another stream of thinking which ²flows on the approach adopted here is that of the ethnomethodological school, especially where their work unwraps the taken for granted structure of scientific method. (McHugh 1970, Wilson 1970, Law and French, McHugh Raffel et al 1974, Pollner 1974). But, I would not want to claim ethnomethodological 'purity' for the paper presented here. I have borrowed (implicitly or explicitly) certain ethnomethodological insights, and used them in, what some might consider, a crude way. In particular, I want to stress, that though the paper's method is microscopical examination, no prescription is thus intended,

indeed, the paper is seen as doing the sort of groundwork, which is needed but has never been done, if the larger aims of the sociology of scientific knowledge are to be carried through in a convincing manner.

2. For sociology see Bell (1974), and also see the demands of parapsychologists reported in this paper - e.g. Fig. 1)

2a For an interesting longitudinal study of scientists without an explicit relativistic orientation, see Mitroff (1974)

3. The change in the state of the embryo from an undifferentiated homogeneous mass of identical cells into potential separate organs. If all the cells are the same at one moment how do they 'decide' which are to become what, and in the right proportions? See Randall (1971) for some discussion of these problems.

3a See p. 6

4. For instance the American publication, Journal of Parapsychology has been criticised for biasing its publications policy in the direction of positive findings, thus disguising the background of negative results.

This type of criticism has been answered by Tart (1973) who argues that the odds against the mass of positive results being due to chance is so high that it is impossible that enough negative experiments could have been done to counterbalance them.

5. This can work the other way too. Often good results may show up in what are counted as preliminary tests, only to disappear in the full scale runs which follow. - Perhaps psi-phenomena are 'shy' for one reason or another.

6. Respondents Backster and Schmidt (see below) both made this point in interview.

7. There is some evidence that self replication may count for more in other controversial fields. David Travis has noticed this in the memory transfer field.

Another interesting point emerging from both Travis' study and my own is the legitimacy of schoolchildren as appropriate replicators. In Travis' study, positive experiments have been reported by schoolgirls, but in my own a negative report was offered by a schoolgirl, Ursula Schwebs (1974), who performed the experiment with the collaboration of her father. Though, to be fair, my critical respondents made little of this, someone must have taken Schwebs' experiment seriously, (or Backster's lightly), for it to be published at all. (It was not published in a heavy-weight journal).

8. Interview material, Russell Targ.

9. Interview material, Professor Joseph Weber, casual conversation with physicists, also Goudsmit (1973).

10. This analysis ~~however~~ of course, has now become a complete caricature of the actual process of the acceptance of new phenomena in science. It must be clear that this kind of situation could never be reached - there are far too many possibilities of arguing about the incompetence of negative experiments or the lack of controls on positive ones, for any commentators ever to agree that such a thing as a series of replications, all of which carried one unambiguous ascription had ever been produced.

11. Another point that has been made by commentators is that, as fraudulent performances should be repeatable at will, whereas genuine performances are not, the high rate of failure is suggestive of the genuineness of the phenomenon.
12. But see Sidman (1966) for such an attempt regarding psychology.
13. It must be pointed out that this experiment is special, in being disliked by many parapsychologists. As has been mentioned in note 4. the Journal of Parapsychology has a reputation for not publishing negative results, yet, they reported the negative replication of the Backster experiment (Johnson 1972) and Backster was virtually ridiculed by the journal's editor J.B. Rhine (1971). Perhaps the idea of plants exhibiting paranormal effects is too much for parapsychologists, who worry about the legitimacy of their discipline at the best of times. One might say that Backster (not even a university scientist but a lie detector expert) is a marginal man's marginal man.
14. Dick Metcalfe and Bob Draper.
15. For instance, see Chedd (1975).
16. In contrast, both Gasteiger and Kmetz took great care over their electrodes, a notoriously problematic element ~~in~~ evenⁱⁿ conventional plant physiological experiments. Silver coated with silver chloride seems to be the best material to use.
17. Here again, the role of experiment as a rhetorical device rather than an objective arbiter is highlighted. See also note 6.

18. Popperians may be tempted to interpret Backster's defences of his experiment as rendering his findings 'unfalsifiable' in the worst sense. Several comments are apposite. Firstly, I have no evidence that any of the actors in this drama were acting in anything other than good faith, indeed, in the case to be described in this footnote, a tactical opportunity was presented to Backster by a mix up on the part of Gasteiger, but no attempt to make capital out of this was pursued to my knowledge. Secondly, an implicit point of the whole paper, is that 'rendering one's experiment unfalsifiable' by the use of ad hoc defences is often indistinguishable from defending one's work from the mistaken attacks of critics. The categorisation of such ploys may depend upon where the categoriser stands in relation to the debate in question. Where the critic sees 'the ad hoc' the believer sees mature understanding and experimental expertise. Nevertheless, perhaps the following anecdote, which involved some fast footwork on the part of Backster's critics may help to make available to the reader, the sense of symmetry and inconclusiveness of the debate which may have been lost in this recital of Backster's purely defensive ploys.

An essential part of the experiment is the control sequence, in which the automatic shrimp dumping apparatus functions exactly as in live 'drops' but the shrimps are absent. At the AAAS symposium ^{a member of the audience} ~~Backster~~ criticised Gasteiger's experiment because tap water was dumped into the killing tank in control drops, and ^{the audience,} ~~Backster~~ claimed that as tap water may very well contain living organisms the plants may have been stimulated by the control drops in the same way as for the live drops and this may have accounted for Gasteiger's lack of results on comparing live with control runs. In the discussion which followed, Gasteiger defended the use of tap water, and later, in my discussion with John Kmetz, I raised this point, and he too argued that this particular objection was not significant.

Now, I have no interest in discussing the 'actual significance' of this point, but it is the case that in the account of Gasteiger's experiment which was subsequently published in Science it is stated that the control drops dumped distilled water into the killing tank. Kmetz, in his defence of Gasteiger's tap water technique was unaware of this later adjustment. Now, my discovery of this discrepancy occurred after my interview with Dr Gasteiger so I was unable to discuss it with him, and so the version of the source of discrepancy which is known to me, is due entirely to Backster. He told me that as far as he now understood Gasteiger's experiments in fact used distilled water in the controls, but that this experimental detail was not ~~remembered by~~ ^{remembered by} Gasteiger at the time of the AAAS meeting, the experiments having been conducted for the most part by his graduate students. This small anecdote is included to show that critics of an unpopular finding may make firm defences of imaginary details of experimental protocol ~~which would be used to defend the validity of the experiment against an opposing viewpoint.~~ ^{where their experiment itself shows the validity of the opposing viewpoint.} ~~experiment actually performed.~~ It would then be quite incorrect to characterise the debate as consisting of 'sound definitive procedures' on one side, and only an ad hoc rescue act on the other.

19. See Collins 1975, where this argument is put into the context of Wittgenstein's discussion of sameness. The repeated experiment may be seen as the scientific institutional equivalent of the use of a concept, in that separate occurrences are seen as manifestations of 'the same thing'.

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