AROUSING PROBLEMS IN PARAPSYCHOLOGY

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Introduction

The experiments to be reported here were aimed at studying different theories of state-correlates of successful psi test performance. There are three such theories important in parapsychology currently: those of Eysenck (1967a), Honorton (in press) and the Brauds (Braud and Braud, 1973, 1974; Braud and Hartgrove, 1976).

Eysenck (1967a, 1975) regards *cortical arousal* as the crucial variable in psi test performance. His theory is of great importance because it is embedded in his all-embracing personality theory (Eysenck, 1953, 1967b, 1970–1) and, if his parapsychological theory were correct, we could infer a great deal about personality and psi. Briefly, Eysenck postulates that (1) low states of cortical arousal are psi-favorable, (2) extraverts have lower levels of cortical arousal than introverts, and (3) therefore extraverts should be superior to introverts on psi tests, Evidence for (1) is presented by Eysenck himself (1975) and by Sargent (1977); for (2), the evidence is presented by Eysenck (1970–1, 1973) and debated by many, e.g. Peck and Whitlow (1975); for (3), the evidence is summarized by Eysenck (1967a) and by Rao (1974). It seems fairly clear that Eysenck is talking of cortical arousal being linked to direction of psi scoring rather than magnitude; from his 1967 paper the way in which (for example) the papers of Astrom (1964, 1965), Green (1967) and Nicol and Humphrey (1953) are cited and discussed leaves little doubt on this point.

Honorton (in press, personal communication) differentiates between *cortical* arousal and *autonomic* arousal in a way which Eysenck does not. Regarding REM sleep as a prototypical psi-optimal state, Honorton argues that psi-optimization is favored by high cortical arousal and low autonomic arousal. He also postulates that psi-optimization is favored by a turning inwards of attention and a

reduction in proprioceptive input to the CNS. Another factor considered to be important by Honorton is the setting in which the experiment is conducted (Honorton, 1974) and for this reason it is not completely clear whether Honorton regards state factors as affecting magnitude or direction of psi scoring or both. His discussion of the hypnosis literature (Honorton and Krippner, 1968; Honorton, 1974) suggests that he views state factors as affecting magnitude of psi effect and other factors, such as interpersonal ones, motivational ones, etc., as affecting direction of scoring. However, in the dream telepathy and in the ganzfeld work, it is clear that psi-*hitting* has been the order of the day.

Lastly, the Brauds (Braud and Braud, 1973, 1974) regard autonomic arousal as a crucial variable in the elicitation of psi, and their use of progressive muscular relaxation has yielded very impressive results. Their position on the role of cortical arousal is not quite so clear. They regard their theory as an extension and elaboration of Honorton's model (Braud and Hartgrove, 1976), but their introduction of a hemispheric-asymmetry postulate (Braud et. al., 1976) makes it unlikely that a simple "cortical arousal" construct would be acceptable to them.

Figure 1 shows four cells which represent experimental conditions which may be produced by varying the two dimensions of arousal, cortical and autonomic.

Cell 1 may be produced with the use of amphetamine or a similar stimulant drug, Cell 2 represents the REM dream state; Cell 3 has not,

		AUTONOMIC AROUSAL LEVEL	
		HIGH (tension)	LOW (relaxation)
CORTICAL AROUSAL LEVEL	HIGH	1	2
	LOW	3	4

Figure 1

I think, ever been researched at all, but it could be done by using sleep-deprived subjects (Ss) with instructions to generate muscle tension; Cell 4 is the experimental condition being attacked here, with the use of sleep deprivation (SD). A brief review of the effects of SD relevant to our considerations will now be given.

Psychologically, SD is known to impair performance on memory tasks (Elkin and Murray, 1974; Kleitman, 1963; Lubin et. al., 1974; Patrick and Gilbert, 1896; etc.) and on vigilance tasks (Daftuar and Sinha, 1972; Deaton et. al., 1971; Froberg et. al., 1972; Hockey, 1970, 1973; Kleitman, 1963; Webb and Agnew, 1973; etc.). This seems to reflect a diminution in the capacity for selective attention: thus Deaton et. al. (1971) found that SD affected the signal detection parameter d' rather than β. Hockey (1970, 1973), using the "observing response" operant technique devised by Holland (1957) for studying selective attention, found that sleep-deprived Ss showed less selectivity in attentional processing than did control Ss. These effects are typically greatest if Ss are faced with a long test and if they cannot pace their own performance: the effects of SD can be counteracted for a short time if S tries to pull himself together, as it were (Kleitman, 1963; Robinson and Herrman, 1922; Robinson and Richardson-Robinson, 1922) but this lasts for short periods only and becomes progressively more difficult for S (it is interesting to compare this with Pearce's reaction to amytal; Rhine, 1934). Electroencephalographically, it is agreed that percent time alpha is quickly and rapidly reduced by SD (Armington and Mitnick, 1959; Blake and Gerard, 1937; Gieseking et. al., 1957, 1958; Johnson et. al., 1965; Jovanic, 1971; Kollar et. al., 1966; Minsky and Cardon, 1962; Naitoh and Johnson, 1972; Rodin et. al., 1962; Tyler et. al., 1947; Williams and Lubin, 1959; Williams et. al., 1959). With increasing length of SD, delta (Blake and Kleitman, 1939; Jovanic, 1971; Naitoh and Johnson, 1972) and eventually theta (Jovanic, 1971; Naitoh et. al., 1969) waves come to dominate the EEG spectrum and the existence of "microsleeps" becomes apparent (Dement, 1974; Kleitman, 1963). There would seem to be a progressive deactivation of the CNS during SD since both contingent negative variations (e.g., Naitoh and Johnson, 1972) and evoked potentials (e.g., Williams et. al., 1964) are diminished by SD.[1] It is also known that SD reduces muscle tension and the ability to exert muscular effort (e.g., Froberg et. al., 1972; Patrick and Gilbert, 1896). We could sum up by saying that cortical and autonomic arousal are both lowered by SD and that attentional processes also seem to be affected.

Some caveats need to be expressed at this point. First, it is known

that some Ss are much more affected by SD than others. Some Ss may show hallucinations after as little as one night without sleep, as did one S in a recent and unreported pilot SD/PK experiment conducted in this laboratory, whilst other Ss may go 11 days without sleep with seemingly no ill effects at all (Dement, 1974). So, if we were to study the effects of SD on psi, we might expect individual differences to show up in our group of Ss. Second, the effects of SD are not simply monotonic or linear. Diurnal rhythms are very often found in data from SD experiments. In the earliest study of SD in man, the authors (Patrick and Gilbert, 1896), noting in detail the behavior of one of their Ss, commented that "The daily rhythm was well marked. During the afternoon and evening the subject was less troubled with sleepiness. The sleepy period was from midnight to noon, of which much the worst part was about dawn" (Patrick and Gilbert's S usually rose at 6 A.M.). The importance of these rhythmic factors has been stressed by later workers studying the behavioral (Loveland and Williams, 1963; Murray et. al., 1958) and physiological (Froberg et. al., 1972; Jovanic, 1971; Kleitman, 1963) effects of SD. Indeed, SD may make cyclic trends in performance on cognitive tasks more prominent than in normal wakeful Ss (Alluisi and Chiles, 1967; Drucker et. al., 1969; Fiorica et. al., 1968; Loveland and Williams, 1963).

Other aspects of SD effects have been heavily researched, e.g., the biochemistry of waking, sleeping, and sleep-deprived states (see, e.g., Dement, 1974; Pribram, 1969) but these are not of direct interest to us here.

Given the overall picture of SD effects, and bearing in mind the other considerations outlined, what might we expect to happen in an SD/psi experiment on the bases of the three theories which we have discussed?

Eysenck's theory clearly predicts that SD should elevate psi scoring. SD lowers cortical arousal and should therefore lead to an increase in psi scoring. Predictions from Honorton's and the Brauds' theories are not easy to draw. One might suspect that Honorton would expect a magnitude affect of SD related to the state-change with other factors determining direction. From the Brauds' theory no clear prediction follows because of the uncertainty about the cortical arousal factor. So, two experiments were conducted to study the effects of SD on (ostensibly) clairvoyant psi, with the intention of investigating the prediction drawn from Eysenck's theory. The first experiment was conducted as a pilot and the methodology was not rigorous, although only psi or fraud could have affected results

as extra-chance agencies. The second, confirmatory, experiment had a very rigorous methodology. Details of these two experiments will now be given.

Experiment I: March, 1976

Method

Subjects

Three Ss, all male "sheep," took part. Two were undergraduates at the University and the third was the experimenter himself. E had not planned to take part as an S, but a potential S declined to take part at the last minute so E stepped in to fill the breach, two Ss only being regarded as too small a sample to be useful. The other two Ss were well-known to E as fellow members of the University SPR.

Procedure

The experiment ran for 36 hours, from 11.00 hours on 3/12/76 to 23.00 hours on 3/13/76. During this time Ss completed a clairvoyant psi test every 30 minutes and also a precognitive psi task every hour. Details of results of the precognitive task will not be given here, because a separate confirmatory experiment was done for the precognitive task and I do not have the space to report that experiment in this paper. The clairvoyance test was a 50-guess run with the targets being randomized digits in the range 1–5 inclusive.

The target digits were computer-generated and pretested by frequency, sequential-dependency, and runs tests before use in the experiment. They were printed out directly onto teletype paper in batches of fifty. E separated these batches and sealed them up inside brown envelopes opaque to a 100-watt light and fluorescent light sources. E passed these target packages to a confederate who numbered them with an arbitrary code and returned them to E, keeping a copy of the code which was only given to E after the completion of the experiment. E was thus blind to the nature of the contents of any of the envelopes during testing and could reasonably take part as an S in the experiment.

During the experiment, the procedure for taking each test was as follows. Ss would sit quietly and record their guesses on standard forms. When all Ss had finished guessing, each S passed his guess sheet to another S. Target packages were then opened and Ss recorded each other's targets on the guess sheet. Ss thus did not record their own targets. It is clear that S fraud was a possibility, however, and

this defect in procedure was rectified in Experiment II. "S-fraud" here would more reasonably be called "co-experimenter fraud" since the Ss were knowledgeable and experienced in parapsychological research.

Ss scored only direct hits during the experiment. After the experiment, target transcriptions and scores on −1, +1, and direct-hitting were checked three times by CLS and a helper.

Nonparapsychological tests

Every 2 hours, Ss filled in a copy of the Bohlin-Kjellberg (1973, 1974) revision of the Thayer (1967) activation inventory. This inventory claims to monitor four separable aspects of psychophysiological functioning: I. Deactivation-Sleep (Sleepiness), II. High Activation (Stress), III. General Deactivation (Euphoria), and IV. Activation (Energy Level; negatively keyed on the inventory). The terms in brackets are those used by Bohlin and Kjellberg (1973) to describe the factor-analytically extracted entities derived from Ss self-ratings, and the unbracketed terms are those used by Thayer. The inventory takes the form of a 23-item nine-point adjective-rating list (a few items from the original Bohlin-Kjellberg inventory were discarded, since the original is in Swedish and some items do not translate well into English).

The inventory has an unstable factorial structure (Bohlin and Kjellberg, 1974) and it was only used in an exploratory fashion here. Little validation of the inventory exists.

Set and Setting Factors

The experiment was conducted in the spacious basement of a private home in Cambridge, and the environment was comfortable and pleasant. Between psi tests, Ss could do more or less anything they wished, but they were not (obviously) allowed to sleep, leave the premises alone, or take alcohol. Caffeine and nicotine were allowed, however. Whilst these drugs may to some extent counteract SD effects on psi, caffeine consumption was not high and only one S smoked. In the case of nicotine, it would certainly have been unwise to have forbidden use of the drug for that would have confounded drug-withdrawal effects with SD effects. Between tests, in fact, Ss read, listened to music, watched television, played board games, talked amongst themselves, and ate frequently (on an ad lib feeding schedule). No attempt was made to regularize meal times; Ss would not have liked that at all.

The general mood and motivation were very good. The three Ss all knew each other well and the experiment was, despite its nature and length, quite enjoyable.

Results

Predictions

No formal predictions were made about the outcome of the experiment. However, E had been able to provide evidence for the validity of Eysenck's theory in previous work (Sargent, in press) and confidently expected scores to incline during the experiment. I mention this since knowledge of E's "expectancy" might be informative.

Results 1. Psi test: Linear trends

Table I shows data from the clairvoyance tests in full.

The exact binomial probability of at least two Ss out of three showing deviations from MCE significant with $P \leq .0096$ on any of the three categories of scoring in either half of the experiment is .00165. The experiment may thus be seen to have yielded strong evidence for the operation of an extra-chance agency affecting results.

TABLE 1

Results of Experiment I

	Subjects					
	G.J.S.		C.L.S.		M.A.	
Half of expt.	1st	2nd	1st	2nd	1st	2nd
Scoring						
−1 MCE = 352.8 Sigma = 16.8	355	307*	349	308**	347	361
Direct Score MCE = 360 Sigma = 16.97	335	367	345	385	375	335
+1 MCE = 352.8 Sigma = 16.8	350	355	360	384	350	324

* $t = 2.74$, $df = 35$, $P = .0096$, two-tailed.
** $t = 2.84$, $df = 35$, $P = .0075$, two-tailed.

t-tests for correlated means do not show that GJS or CLS showed a significant decline from first to second half of the experiment, and thus the results show significant psi-missing in the second half of the experiment and essentially null data for the first half of the experiment. One notes that GJS and CLS show exactly the same trends over time (lower −1, higher direct-hit, higher +1 scoring) whilst MA shows exactly the reverse picture. This has a P-value of .033 which is mildly suggestive.

Secondary analyses of the data will be presented together with the results of secondary analyses for the data from the confirmatory experiment.

Results 2. Activation inventory

Using the item loadings recommended by Bohlin and Kjellberg (1973) it was found that raw scores on Factors I, III, and IV intercorrelated with mean $r_s + .80$, whilst their mean intercorrelation with Factor II was −.52. For this reason, the global parameter (I + III + IV − II) was used for the purposes of correlations.

Correlations of this global parameter with time-of-testing for the three Ss were:

G.J.S. −.64, $P < .01$, one-tailed.

C.L.S. −.46, $P < .03$, one-tailed.

M.A. −.22, P N.S.

Thus we see that the Ss who showed psi effects were more affected by SD than the S who did not. However, we may not take this finding at face value. Whilst Ss did not compute −1 scores during the experiment, it could be argued that they unconsciously noticed them and that this affected their ratings on the inventory. If one correlates the (I + III + IV − II) parameter with the −1 scores on the four tests taken *after* filling in each copy of the inventory, the correlations do not reach significance (+.26 for CLS and +.43 for GJS) but then we would not expect them to do so, since the data does not show a significant *shift* from first to second half of experiment. Correlations for individual factors with −1 scoring are rather weaker than those found between −1 scoring and the global parameter, which suggests that both autonomic (Factor II) and cortical (Factors I, IV) arousal are adding to the effect. However, since no significant shift was found in the data from the psi test it is not surprising that the results of the activation inventory are not very illuminating or dramatic.

Conclusions

The results of the experiment went contrary to Es expectation and a tentative conclusion was that Eysenck's theory was contradicted by the data. However, it was clear that a replicative study needed to be undertaken. This experiment will now be reported.

Experiment II: December, 1976

Method

Subjects

Four Ss took part in the experiment, three male and one female, all sheep. CLS took part as a fifth S out of personal interest but it was not planned to include his data in a formal statistical evaluation of results. The experiment was designed to be proof against E-fraud and so CLS, who was in charge of target preparation, could hardly participate as a S. Ss were well-known to E and were members of the University SPR. Three of them were undergraduates at the university and the other was a student at a technical college.

Procedure

The experiment ran from 11.00 hours on 12/6/76 to 23.00 hours on 12/7/76. The procedure followed that used in Experiment I with the following changes:

(1) No precognitive task was used. In Experiment I, the theoretical possibility of some "preferential" or "differential" effect operating on the two tasks was present, although none was in fact observed. In this experiment, it was deemed desirable to exclude this possibility.

(2) Data from the activation inventory was not studied because of the lack of predictive power in Experiment I. The inventory was only used to replicate the conditions of Experiment I.

(3) Target packages were loosely concealed inside larger brown envelopes during testing. There was no doubt about the opacity of target packages in either experiment, but a still further protection against sensory cues (or tampering) could not do any harm.

(4) When recording their guesses, Ss made carbon copies of them, and placed these in a large box directly in front of E before they were allowed to open their target package. The box never left E's field of vision. This effectively eliminates S-fraud.

(5) At the end of the experiment, a helper collected all the data with CLS and sealed it up inside a suitcase. The case had an

envelope bearing the legend "UNIVERSITY OF CAMBRIDGE" stuck to it and was taken by the helper to Christ's College, where it was taken into custody by the porters and signed in, the time and date being recorded on it. The helper retrieved the case on 12/9/76 and brought it to CLS. In the presence of two witnesses, the data was then sorted out and Xeroxed. The Xerox copies—which could not be tampered with—were signed by the witnesses. This procedure effectively eliminates E-fraud.

(6) Three checkers checked the data, rather than two as in Experiment I.

Set and Setting Factors

The experiment was conducted in the same locale as Experiment I. The same restrictions on Ss activities were placed as in Experiment I. Ss filled in the time between tests much as previously.

This experiment was much more of a strain than the first study. One subject became quite badly ill during the early hours of the morning of 12/7/76, suffering severe gastric upset and shivering fits. It was very hard for him to stay awake but he did not ask to leave the experiment. Another S became rather emotional later that same morning.

Results

Two predictions were made about the outcomes of the experiment; the second one will be discussed with reference to secondary analyses of data, but the central prediction was that individual Ss would show significant −1 psi-missing in the second half of the experiment.

Results 1. Psi test: Linear trends

Table II shows data from Experiment II in full.

Two of the four Ss show the predicted second-half −1 psi-missing effect. The exact binomial probability of at least two Ss out of four confirming the experimental hypothesis at $P \leq .029$ is .0049. One of these two Ss, J.S., also shows a marginally significant decline from first to second half of experiment on −1 scoring (t-test for correlated means yields $t = 2.12$, $df = 35$, $P = .042$, two-tailed since this was not predicted).

R.S. was the S who fell sick during the morning of 12/7/76. He shows low −1 scoring in the first half of the experiment, but this

TABLE 2

Results of Experiment II

	Subjects							
	H.A.		A.P.		J.S.		R.S.	
Half of expt.	1st	2nd	1st	2nd	1st	2nd	1st	2nd
Scoring								
−1 MCE = 352.8 Sigma = 16.8	328	364	345	315*	375	320**	317***	345
Direct Score MCE = 360 Sigma = 16.97	346	346	360	356	383	355	365	372
+1 MCE = 352.8 Sigma = 16.8	361	363	338	343	350	349	370	320

* t = 1.98, df = 35, P = .028, one-tailed.
** t = 1.96, df = 35, P = .029, one-tailed.
*** t = 2.60, df = 35, P = .014, two-tailed.

could be due to chance; by now, 21 first-half-of-experiment scores had been collected, so one of them deviating from MCE with P = .014 is not significant.

CLS's data is not given here but he did not show the second-half −1 psi-missing effect. This could have been due to his cultivation of sleep deprivation as a way of life in the weeks preceding the experiment, and/or to the stresses and strains he bore as E here: worrying about RS's health, checking that all guess-carbons were in the box before any target packages were opened, worrying about JS's minor emotional upset, etc. These factors, absent in the first experiment, gave CLS little chance to try and get into a psi-conducive state of mind.

Secondary analyses of data will now be reported and then a discussion of the implications of results given.

Secondary Analyses of Results from Experiments I & II

1. Cyclic trends in scoring

Figure 2 shows the −1 scores of the four Ss who showed psi effects arbitrarily summated into four-hour epochs and plotted against time. Three of the Ss show a clear dip in performance at the

time of normal waking (8 A.M. for GJS and AP, and 1 P.M. for CLS, who is nocturnal when circumstances permit). GJS shows a perfect diurnal rhythm of scoring, peaking in the late evening and reaching a nadir in the early morning. AP and CLS show no clear peak in scoring but, like JS (and unlike GJS) they show a marked dip at the end of the experiment. JS shows no rhythm of scoring.

No attempt has been made to submit this data to formal analysis—for example, Fourier analyses (Sollberger, 1965, 1967). It would be premature to do this, and we would need more than one cycle per S to do it in any case (and consequently would need to carry out a 60-hour or 72-hour experiment, or to test these Ss again). Two lines of evidence suggest that these features of scoring are genuine, however. Referring to the precognition work (two experiments have been carried out into SD effects on precognition here) the same features persist, and GJS showed very clear late-evening peaks in the precognition task also (for GJS, the correlation between scoring on the clairvoyance and precognition tasks was +.47 which is significant with P = .046 two-tailed). And in these clairvoyance experiments, data from another type of secondary analysis supports the contention that these rhythmic features in scoring are not artifacts.

2. Position effects in scoring

When data from Experiment I were analyzed, it was found that both GJS and CLS showed the −1 psi-missing effect focused on the last 25 trials of the run (Table III). It was thus predicted in the

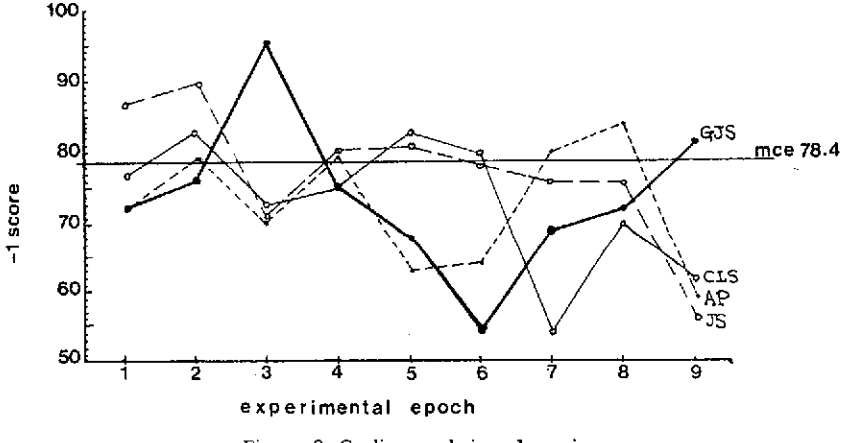

Figure 2. Cyclic trends in −1 scoring.

TABLE 3

Position Effects in −1 Scores of "Successful" Ss

Subject	Deviation Scores	
	First 25 trials	Second 25 trials
GJS	−12.8	−33*
CLS	−10.8	−34**
AP	−5.8	−32***
JS	−18.8	−14

* t = 2.55, df = 35, P = .016, two-tailed.
** t = 4.27, df = 35, P = .00014, two-tailed.
*** t = 2.40, df = 35, P = .011, one-tailed.

second experiment that Ss who showed the −1 psi-missing effect would show the same trend. One of the two Ss, AP, fulfilled this prediction with P = .011.

Thus, the three Ss who appeared to show cyclicity of scoring also show the end-of-run psi-missing focusing effect. This "focusing" is very significant for CLS because the empirical standard deviation of his second-half-of-run −1 scores is not the theoretical 2.00 but is 1.33. Indeed, during the second half of the experiment CLS managed to get a run of 25 successive tests without an above-chance second-half-of-run −1 score.

Ss who show cyclic trends in performance also show this end-of-run focusing, which suggests some common basis for the two phenomena.

Discussion and Conclusions

The effects of SD on clairvoyant psi seem to be replicable and orderly and the P-values noted are reasonably small. It appears that SD might be a promising addition to the parapsychologist's armory of experimental manipulations. It is necessary, I think, to try to separate out the general from the specific in considering the implications which might be drawn from the results, and here it is necessary for me to refer to the results from two sets of SD/precognition experiments as yet unpublished.

The first precognition experiment, which ran in parallel with the first clairvoyance experiment, yielded evidence of significant declines in scoring with SD, and the two Ss who showed this trend were GJS and CLS. The overall results of the study were significant with P = .0067. In a second experiment, using a single S, four

conditions were used: two control conditions and two quite separate sleep-deprived conditions. In that experiment, declines in scoring on −1 displacement were noted, which (considered as the best of three scoring schemes) were only significant with P = .033.

I regret having to make reference to unpublished data, but space does not permit a full report on these precognition experiments here, whilst a consideration of them is essential for an appraisal of the results from the clairvoyance work. In these clairvoyance experiments, essentially null data from Ss became psi-missing data under the influence of SD. In the precognition work, Ss showed some tendency to psi-hit under the control condition and a significant *difference* between control and sleep-deprived conditions was noted. Pieced together, the results take the form shown in Figure 3.

From this overall picture, it would seem that SD affects the direction factor in scoring rather than the magnitude factor. If one were to consider the clairvoyance work in isolation, it might seem that SD affected magnitude as well, since significant deviations from MCE were only found under the sleep-deprived condition.

The precognition work is also of importance in another connection. The second precognition/SD experiment used a control-SD-SD-control counterbalanced design, rather than one 36-hour test, which eliminated the learning hypothesis of results. From the clairvoyance work alone, it might have been argued that Ss were "learning to psi-miss," although such an explanation would not have accounted for the focusing effects noted (which suggest an attentional interpretation of results rather than a learning explanation) or the periodicities in scoring (which suggest a physiological basis for SD

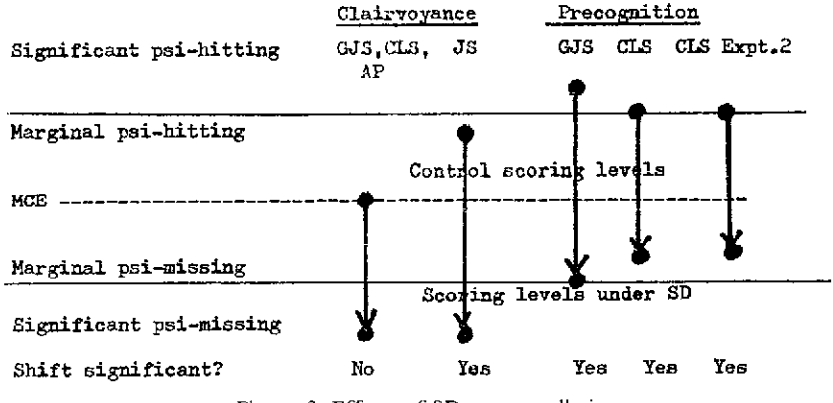

Figure 3. Effects of SD: an overall view.

effects on psi). Results from an adjective check-list used in the same second precognition experiment showed that ratings of boredom did not differ significantly between control and SD conditions, and hence the motivational explanation of results was ruled out (such an explanation would run into difficulties with the clairvoyance work since motivation and mood were worse in the second experiment than in the first, but −1 scoring rates were lower in the first experiment than in the second).

I think the results taken overall do show that SD affects the directional factor in psi and lowers scoring. Further, it is difficult to give an explanation in terms of learning effects or motivational factors to account for the basis of SD effects, and so a state-change and/or an attentional explanation would seem most promising.

An attentional interpretation of results is strongly suggested by the end-of-run focusing effect. Ss try to focus attention on the task in hand at the start of a test, but attention and concentration cannot be maintained and psi-missing sets in later in the run. In the precognition work, GJS's significant shift was found to be localized at the end of the run, where it was highly significant ($P = .00005$), which provides further support for this hypothesis; but this feature was completely absent in the CLS data, as it was in the JS/clairvoyance data. It thus seems likely that attentional changes are consequent upon state changes induced by SD, and may in some Ss play an important role in mediating changes in psi scoring, but not always so.

The state-changes brought about by SD clearly include negative shifts in both cortical and autonomic arousal, as expected. It seems likely that both of these changes play some part in the changes in psi scoring noted, but, as we have seen, it is their interaction which is of the greatest interest, and these experiments have to be viewed in the light of others if we are to draw any conclusions about their relative roles in influencing the operation of psi.

Returning to the four-cell model shown in Figure 1, we now find ourselves able to say something about three of those four cells (Figure 4).

I want to say at once that I think Figure 4 is an oversimplification. Attentional factors are related to cortical arousal level but are to some degree tangential to that parameter, and there is the distinct possibility that at least *two* cortical arousal factors, pertaining to right and left hemispheres, might be taken into consideration. But, at least, we can say something from Figure 4 and the data summarized in it, and a lot of possibilities for fruitful research can be based on it.

CORTICAL AROUSAL LEVEL	AUTONOMIC AROUSAL LEVEL: HIGH	AUTONOMIC AROUSAL LEVEL: LOW
HIGH	Psi-missing/ negative shift in scoring*	Psi-hitting/ positive shift**
LOW		Psi-missing/ negative shift***

* Amphetamine effects: Sargent, in press.
** REM Sleep, Ganzfeld states etc; Honorton.
*** These experiments.

Figure 4.

One thing we may do is eliminate simple models of the sort "low cortical arousal = psi-hitting" and "low autonomic arousal = psi-hitting." We may consider that Eysenck's hypotheses are contradicted by the data presented here, for example. So some possibilities may be eliminated.

A second possibility from Figure 2, which almost cries out to be researched, is the missing cell: low cortical arousal and high autonomic arousal. This wouldn't be easy to produce, since high autonomic arousal would tend to elevate cortical arousal via proprioceptive input to the cortex, but with sleep-deprived Ss—where the reactivity of the CNS is diminished—you could have Ss generating muscle tension and see what happened.

Whilst as yet we have no replicative studies of SD effects, the results reported here confirm a hypothesis stated by Honorton (1974) as follows: "Relatively large and rapid shifts in state will be associated with enhanced ESP performance," to which is added "It is not clear whether this proposition should be stated in terms of *directional* shift" (Honorton, 1974, p. 55: his italic). Whilst SD-induced shifts are not rapid, they are certainly large. So this work, whilst novel, confirms the idea that psi effects may be elicited if large state-shifts are used.

Having already suggested one possibility for research-attacking the "missing cell" of Figure 4, many others might be noted. I should state that all my suggestions and conclusions bear only on tasks in which ESP is a possibility (rather than pure PK tests) since a first SD/PK experiment conducted here (which needs replication) yielded a significant incline in results—another result from an SD experiment which went contrary to the experimenter's expectations. So, if one

were using an SD/ESP test design, the following possibilities suggest themselves.

First, for the sake of purism, separate Ss might be used in control and sleep-deprived conditions. These experiments used same-Ss designs which allow for the possibility of differential effects in the Ss reactions. This possibility is strongly argued against logically. The division of the experiment into two halves was simply a convenient way of treating data and psychologically there weren't two conditions for Ss, and the use of a between-Ss design gives you the problem of group-matching, but such an experiment could be done.

Much more interesting and important would be studies undertaken to examine the physiological correlates of psi performance under SD. EEG studies would be one possibility, and a long duration SD experiment coupled with monitoring of endocrines known to show diurnal fluctuations in plasma level—e.g., the corticosteroids (Conroy and Mills, 1970, pp. 31–37) and catecholamines (Karki, 1956; von Euler, 1956; von Euler et. al., 1955)—would be another. Amphetamine might be given to sleep-deprived Ss to see what effects that produced. Clearly much could be done.

Karl Lashley is said to have stayed up all night guessing ESP cards after reading J. B. Rhine's "Extra-Sensory Perception." He got high scores, by the way, but it wasn't a controlled experiment! I doubt whether legions of parapsychologists might do something similar after reading this, but I hope one or two of them might.

FOOTNOTE

1. I am not neglecting the literature of EEG activation via SD; however, the activation of the EEG by SD has been reported (e.g., Pratt et. al., 1967) in groups of epileptic Ss, hardly a good population to make generalizations from.

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DISCUSSION

HONORTON: Carl, I notice in your Table 2 on the second experiment that one of your subjects, *R.S.*, actually did show a significant increase in scoring and in fact, that is considerably stronger than the decline that the other two subjects showed.

SARGENT: That's not strictly true. The only person who shows a significant shift at all is *J.S.* That's a 55 point shift. *R.S.* changes by 28, so J.S. shows twice as large a quantitative shift. He's got the biggest deviation in the whole batch in the first half. That is interesting, only it's a great shame that this is the subject who during the early hours of the morning cracked up and became sick. Because now we've got a totally aberrant piece of data. We don't know what to make of it. Who knows what would have happened if he had been okay and continued? Yes, that is odd. I can't explain that. But you see, again that might just be a random fluctuation when you

consider all the material. We've got 36 sets of scores. Something's got to come out somewhere as well as the stuff that's been coming out consistently, because what we've got out of these 36 scores is 4 minus 1 second half deviations that are significant both predicted and non-predicted. You've got 32 other sets of scores here. This could just be the one that comes out. It's quite possible. It could be just a random fluctuation.

DEAN: I'm interested in the diurnal effect. Would you describe it a little bit more.

SARGENT: Well, one thing that we've got there from three of the subjects who are showing this effect, is a pretty good dip just at or around the time of normal waking, which looks lawful. This is what you get elsewhere. It suggests to me the possibility that you might start having a look at some physiological factors. And I've drawn the most convincing one with thick black lines, you know, it's standard. You always do that with your best result. And that's a beauty—the difference between the top and the bottom of the curve is over five standard deviations. That's a huge effect. Even if it's only one out of seven or eight subjects, that's a very big effect.

TART: I don't know if you know the paper on "Transtemporal Inhibition" I gave at the P. A. Conference last week, but basically what I have found in looking at the results of high scoring percipients is that people who hit on the real time target in a GESP experiment tend to miss on the immediately upcoming target, the +1, precognitive target. This is usually the case, except that sometimes there is a shift. It's as if they haven't quite focused their psi and they may occasionally shift to hitting on the +1 target and then missing on the +2 and the real time targets. Your data makes me think that with the varying levels of activation here, especially with a diurnal cycle running through, you might find some very interesting cyclical effects as to where the shift of the temporal focus of hitting versus an immediately surrounding missing is. This missing is an effect like lateral inhibition in the nervous system, but it's extending over time.

SARGENT: Didn't you have some problems with over-alternation of the generator in that data?

TART: No, I had my problems with my consulting statisticians more than problems with the data.

SARGENT: If you have over-alternation that would explain why if you have missing, you've got an apparent precognitive miss.

TART: No, I don't have over-alternation—that's a very technical argument I can talk to you about later.

SARGENT: It couldn't explain your number of hits, but it could possibly explain that point that you're making.

TART: It can't explain the misses either. (A full explication of the transtemporal inhibition effect referred to here and the data and analyses to answer Mr. Sargent's question may be found in my Presidential Address, "Space, Time, and Mind," which will be published shortly in *Research in Parapsychology 1977*.)

SARGENT: Such an analysis would be feasible if and when I get a tame computer man to transfer the slot to cards for me. This is a lot of data, you know. You've really got to see the stacks of paper and you're instantly depressed.

TART: There's a problem with focusing on tests when people are sleep-deprived, so you might very well have shifting focus in the psi test.

SARGENT: Well, you see, there is something very paradoxical about these findings. It's suggestive that the focus of selective attention is impaired by sleep deprivation, but that's not what we've got here. We've got a sharpening up focus on the mind as well. I don't know why.