p. 463, l. 28

SPERRY: I interjected, Bob, because I have never been entirely satisfied with the materialistic, or behavioristic thesis that a complete explanation of brain function is possible in purely objective terms with no reference to subjective experience, i.e. that in our scientific analysis we can confidently, and advantageously, disregard the subjective properties of the brain process. I don't mean that we should abandon the objective approach nor repeat the errors of the earlier introspective studies. It's just that I find it difficult to believe that the sensations and other subjective experiences serve no function, no operational value, no place in our working models of the brain, blackboard, or otherwise. The materialistic dialectic advanced by Bechterev, Pavlan, Watson and others is still not completely foolproof; there remains a weak link, deep centrally between input and output, perhaps about where the impulses hit those positive and negative (self) reinforcement centers that Dr. Olds and Dr. Lilly and others are mapping. Perhaps it is true that the 'pain' and the 'purple glow' effects of the self-activated electrode can be ignored in our explanatory neural models, but in my book the point is not satisfactorily settled.

p. 464, l. 17.

With reference to the conditioned response, specifically, I suspect that a good case can be made for the contention that in most or all conditioning, the stimuli used, in order to be effective, must register as sensation or feeling in the neural stream of subjective awareness. In other words the animal must feel the pain from the shock, must smell or taste the meat juice, and so on. Most of us proceed on the familiar and generally accepted thesis that these subjective phenomena play no part in the causal sequence. Our picture of how the brain excitations are generated and transmitted has no place where a sensation, the subjective property, i.e., could get into the act.

On the other side, is the argument that the pain per se, and subjective awareness in general, emerged in central nervous evolution and must have been maintained and differentiated because it does serve a real use, i.e. becaus of its operational value in the causal sequence. On these terms any physiological model of the conditioned response that fails to include the subjective properties is bound to end up with some kind of gap in the chain of cerebral events. My point is merely that we may have gone a bit far in the past several decades with our behavioristic postulate that the science of neurophysiology can confidently assume a full understanding of cerebral events is possible in theory from a purely objective approach that excludes subjective awareness.

p. 464, 1. 29

SPERRY: Delete.

p. 464, 1. 31

SPERRY: Delete (delete whole passage 1. 27-32?)

center of the natural reflex. Dr. Liddell has mentioned that ~~he thinks~~ no ~~good~~ really adequate brain theory has been ~~suggested~~ brought forward to replace Pavlov's, ~~or at least, nothing adequate.~~ At most we have ~~had~~ only some vague thinking about the possible nature and location of the new connections laid down between ~~the~~ conditioned stimulus and response centers; ~~to the effect~~ i.e. that they must ~~at least~~ be more complex than the direct transcortical linkages proposed by Pavlov, that probably they involve subcortical centers, and that ~~possibly~~ some kind of reverberatory activity ~~is~~ may be important in the earlier stages (L).

Some years ago (M) I stuck my neck out to suggest that the conditioned reflex does not necessarily depend upon the establishment of any type ~~of~~ of traces or connections between the~~se two brain~~ CS-CR centers, ~~but that~~ the neural association between conditioned stimulus and response ~~is~~ was conceived to be a purely functional one ~~and is~~ and effected in quite a different way (M) but this ~~--which is~~ is probably too long a story to go into now. ¶ Well, Briefly, the suggestion is that the engrams support the arousal of ~~an~~ a perception or 'expectancy' of what is to coming in the conditioning situation. Having learned what to expect the animal prepares through a 'cerebral facilitory set' to make the appropriate response. ~~In instrumental conditioning, this leads to the establishment of a preparational facilatory set.~~ The excitations of the conditioning stimulus, ~~then,~~ are routed into the new pathways of the CR not by left over traces but by an active ~~by the existing~~ pattern of facilitation and inhibition imposed on the neural circuits by th~~is~~ transient facilatory set. With~~in~~ this scheme there is no need to search for the new 'connections' established between conditioned stimulus and response centers, as ~~has been~~ almost universally assumed, because there are none there. There is only an evanescent opening or facilitation of these (preexistent) pathways ~~in~~ within the conditioning situation. The permanent traces that lead to arousal of

Enclosed are further corrections and references making mine complete through page 440. I will forward the remainder very shortly.

R.W. Sperry

p. 150
l. 6 SPERRY: Delete this whole p. . . .

3 A

insert from p. 152 / 235

p. 150, line 6: Delete this whole passage: line 6 - 15 and line 18-19.

— — — —

P. 163, line 15:

SPERRY: I don't want to change the subject, but--

p. 163, line 18:

SPERRY: Would you say, Dr. Liddell, that there has been ~~any novel or~~ any significant development~~s~~ in the brain theory of conditioning since Pavolv's time?

p. 167, line 17:

SPERRY: ~~By way of reflection~~ In thinking back at this point, I'm concerned that we ~~may~~ could be leav~~e~~ing an impression that Pavlov's theory has ~~been~~ remained the accepted and prevailing physiological explanation of the conditioned response up to the time of the ~~a~~ recent implanted electrode studies.

p. 167, line 22:

SPERRY: I would have guessed that his conceptions of irradiating excitation and inhibition have been considered quite inadequate for at least twenty years.

p. 167, line 27: delete lines 27 - 29.

p. 176, line 17:

SPERRY: Has anyone tried to establish these low-level, visceral-visceral conditioned reflexes in decorticate animals?

p. 152, line 29

SPERRY: There are reports of spinal conditioning in the frog by Franzisket in Rensch's laboratory that seem to be pretty good. In chronic spinal frogs they pair a strong and a weak cutaneous stimulus. The stronger stimulus, to the flank, say, dominates during the conditioning trials and inhibits a response that otherwise would occur to the weaker stimulus, say to the forelimb. (This differs from the usual procedure in which an indifferent or neutral stimulus is used as the conditioning stimulus.) After several hundred pairings, application of the weaker stimulus to the forelimb ~~alone~~ evokes by itself the flankwiping reflex of the hind leg instead of the normal forelimb response. The conditioned responses show an early labile phase with a chemical-like waning and a more lasting stable phase after many pairings up to 1500 or more applied over 75 to 100 days. Rensch and Fransisket (D.5) appear to have answered the objection that they are dealing merely with temporary heightened excitability and irradiation phenomena. The spinal sections at the base of the medulla were confirmed histologically. For some reason they have success with waterfrogs but not with some other species.

Line 35

SPERRY: Before we leave Pavlov, there is one other minor point. I recall that in Pavlov's lectures he inferred from the cutaneous and auditory conditioning observations a precise topographic mapping of these sensory fields in the cortex. Is there any possibility that this antedated the direct anatomical and physiological mapping. Does anybody know?

P. 153

Line 8 SPERRY: Do you know whether this preceded the more direct anatomical demonstration of the topographical detail?

Delete Lines 11 and 12.

p. 235

Line 22 SPERRY: If it should be true, as seems likely, that your central stimulation here, in order to be effective has to evoke a sensation, auditory, visual, or whatever, depending on the area you are in; then it is possible that, with a peripheral stimulus, one would have better control over the exact nature and even the intensity of this evoked sensation than is possible with this method.

p. 185, Delete lines 15 - 27.

p. 196, Delete lines 14-18.

p. 208, line 2:

SPERRY: This is the first time I have ever been called a neo-Pavlovian. (Laughter)

p. 208, line 6:

SPERRY: To continue the line of discussion here, I think perhaps Dr. Olds was referring to some work that we ~~have done~~ did a few years ago in an effort to test the possible functional role of intracortical transmission as postulated in Pavlov's scheme. Briefly the experiments consisted of placing in the cortex numerous intersecting knife cuts, ~~or~~ inserts of tantalum wire, or dielectric plates of mica in such a way as to block, or at least to grossly distort, the patterning of any tangential intracortical transmission. Although we were not aiming particularly at Pavlov's concept of irradiation, I think that the absence of any significant functional disorganization as a result of these measures, ~~to our mind at least,~~ ~~is pretty disconcerting to~~ ~~pretty much eliminates this kind of~~ is difficult to reconcile with any such concept. We never emphasized this specific point, because, I had supposed that the idea of cortical irradiation had already been pretty much abandoned for other reasons.

~~I too have had the feeling that in~~ Now that we are approaching the new implanted electrode work, I wonder if it would not be helpful, particularly for those of us not working on conditioning, to ~~have as a background a summary of~~ summarize briefly some of those conditioning phenomena that have seemed particularly relevant to brain theory. I can

p. 208, line 23 (cont'd)

start by mentioning a few that come to mind and probably others here can add to the list.

First, I think we have not yet mentioned conditioning under curare, ~~which~~ Apparently the process goes ~~proceeds~~ perfectly well in the absence of any motor response. The motor response has been eliminated also, I believe, by crushing of the nerves, and further, by local anesthetization of the motor cortex, which, of course, blots out the--[Doty says "No."] Well, you correct me on that.

p. 208, line 35:

SPERRY: ~~Well,~~ In any case, there have been experiments in which ablation of the motor cortex has failed to abolish learned responses (E). This should eliminate as a necessary part of the brain mechanism, the dominant focus of attraction in the cortex that according to Pavlov was supposed to funnel the conditioning stimulus excitation down into the reflex motor pathways.

The effect of random ~~aperiodic~~ reinforcement is particularly critical for any brain theory. We estimate the strength of the conditioned reflex in part by its duration and the difficulty of extinguishing it. It has been shown (F) that with an equal number of trials in the conditioning procedure, aperiodic, rather than regular reinforcement ~~with every trial,~~ produces a CR that is much more difficult to extinguish than ~~is~~ is the CR formed with ~~regular~~ reinforcement at every trial. According to most of the physiological explanations, ~~including that of Pavlov,~~ you ~~should~~ would expect to get a much stronger connection between the brain centers involved if you ~~reinforce every time~~ ~~pair the conditioned and unconditioned stimulus~~ pair the unconditioned with the conditioned stimulus on ev tri.

The effect of alternate reinforcement and extinction has already been mentioned. If you ~~set up~~ establish a ~~conditioned reflex~~ conditioned response, then extinguish it thoroughly ~~through 100 trials, and~~, then reestablish it, and then extinguish again, ~~you~~ it has b

p. 209, line 22 (cont'd)

found that, after so many repetitions of this, ~~that~~ the ~~conditioned reflex~~ learned response can be reestablished with a single trial (G). This too has important implications for the underlying brain process.

Just the ~~pen~~ phenomenon of ~~the~~ delayed ~~CR~~ conditioning is interesting. Generally, the signal stimulus precedes the natural reflex by a short period, from, say, a half second which is about optimum for the eyelid response in man on up depending on the situation and species. It is possible to set this signal stimulus as far forward as a half hour or maybe even longer. This poses some nice physiological problems as to the nature of the trace effects of the stimulus and how they operate at the end of the delay. The animal somehow has to hold the effect and to respond at the proper time. It is similar or very close to the so-called "timing behavior" that Galambos and Morgan refer to in their forthcoming chapter in the 'Handbook.'

Even the simple absence of reversed conditioning is something to keep in mind in formulating a brain theory. That is, that the signal stimulus has to precede the reflex that you are going to tie it to. I wouldn't be surprised if there exists in the vertebrate brain in general some kind of a built-in tendency to perceive 'what-follows-what,' 'what-leads-to-what'. Appropriate central readjustment ~~response~~ in this respect is fundamental not only to our cause-effect thinking, but to the behavior of all vertebrates from the lowest forms to the highest.

In particular, we should keep in mind examples of rapid conditioning. In conditioning, we have ~~the~~ one problem ~~of~~ in the acquisition of the conditioned reflex, and another associated with ~~then as a separate problem that of~~ its prolonged retention. Generally it is ~~generally~~ not easy to distinguish ~~easy to confuse~~ the two because in most laboratory conditioning the time span is great enough so that the acquisition proceeds in part on the basis of traces retained from earlier conditioning trials. However, ~~I~~ it

p. 210 (cont'd)

is important to remember that a great deal of rapid conditioning and learning can and does occur in a single trial or two, not only in the laboratory but also under natural conditions. In working with human subjects, particularly, it is not difficult to establish a conditioned reflex and then to extinguish it, all within ~~in~~ a twenty-minute session. A lot of learning and conditioning is so rapid that you don't have to deal with the permanent-type memory traces at all. The establishment of the 'temporary connection' becomes a ~~It becomes a~~ problem primarily of dynamic reorganization. ~~This is what establishes the new connection~~. In time the reorganization becomes ~~is~~ consolidated through ~~and~~ lasting tissue changes. ~~occur to effect retention~~. For convenience in dealing with the brain changes, ~~conditioning~~ we can separate these two phases of the ~~process~~ phenomenon:

~~There are these two aspects of conditioning that can be distinguished~~ the reorganization process, and ~~then~~ the tissue changes for retention. ~~by which the reorganization is consolidated and retained~~.

The effect of ~~the~~ electroconvulsive shock is of interest in this regard ~~for brain theory, the fact that~~ in that electroconvulsive storms wipe out temporary or recent learning, i.e., of trials made up to a half hour or so before the ECS, but do not ~~xx~~ eradicate the more permanent trace systems, ~~effects~~.

p. 211, line 1:

SPERRY: Yes.

p. 211, line 12:

SPERRY: Yes, it is particularly relevant to the problem of the engram and its nature. Another point here will illustrate~~ing~~ the rapid dynamic reorganization occurring independent of trace formation. ~~I recall one~~ Experiments with ~~in~~ human subjects (A) have ~~it has~~ shown that ~~conditioning in which~~ a conditioned response that required some 16 to 20 ~~twelve~~ trials to establish under the usual conditions, will be ~~was~~ performed

p. 211 (cont'd)

on the very first trial with no training when the subjects ~~were~~ are given a full understanding of what to expect in the conditioning procedure.

p. 211, line 21:

SPERRY: Yes, this ~~is~~ was in man. ~~The effect of intervening activity between trials is important. Experiments have been done in which the attempt has been made to wipe out all intervening activity that might tend to obliterate the traces of preceding trials. The question is whether you get better retention under these conditions,~~ ~~and I believe this is the case.~~

replace by insert (Y)

¶ ~~There is also the data on~~ With respect to decorticate ~~and spinal~~ conditioning ~~that I think we have mentioned only briefly~~. It is worth ~~remembering~~ noting here that fishes ~~have~~ show excellent learning and retention after removal of the entire forebrain.

~~In this connection~~ Dr. Arora in our laboratory has recently confirmed this. ~~and that~~ We find also that a visual discrimination can be retained in fishes after complete section and regeneration of the optic nerve. This showing shows that the ~~new connections~~ memory traces or ~~traces~~ engrams are not rigidly or directly connected to the sensory input channels. There probably is ~~some~~ a certain amount of reshuffling of optic fiber connections in the brain as a result of regeneration. We ~~think~~ infer that the regenerated fibers get back pretty close to the same cells, but suppose that probably they do not reestablish exactly the same synaptic terminals. Whatever the degree of synaptic rearrangement, it does not disturb reactivation of the engram.

p. 212, line 7:

SPERRY: Yes. These are color and also acuity discrimination habits. The findings show not only that memory for the habit is retained, but also that color perception is restored in its original form after regeneration. The restored visual acuity also approximates closely that of the ~~and it becomes necessary to~~ one must infer the existence of another dimension of specificity among the optic fibers associated with color. ~~and~~ This presumably is superimposed upon the topical specificity demonstrated earlier and associated with directionality (Y).

NASSAU TAVERN HOTEL

PALMER SQUARE

PRINCETON, N. J.

INSERT Y on p. 211

~~motor equivalence~~. An animal will easily and spontaneously substitute for the conditioned response a quite different response if the situation is changed to ~~demands~~ it, or if the goal is perceived to be more readily achieved thereby. There is, of course, continuous motor readjustment of this kind in the learning of new motor skills.

I don't recall ~~argument of the~~ that we have discussed ~~phenomenon of~~ motor equivalence as seen in instrumental conditioning. It is also difficult to account for with any theory that postulates the wearing of connections between CS and CR centers. The observation in this case is that an animal will....

.A1[2]

Insert R

~~In this connection~~ One ~~wonders why~~ may wonder here whether the impulses generated ~~by~~ in electroconvulsive ~~therapy~~ shock are as ~~such~~ effective ~~in establishing~~ in establishing traces ~~[illegible]~~ as are impulses generated in organized activity. ~~I don't know but that they are.~~ If repeated frequently enough, entirely possible that ~~[illegible]~~ ECS treatment does gradually wear blankness and ~~disorganization~~ confusion into the brain the traces for ~~[illegible]~~ which in time ~~[illegible]~~ begin to ~~replace~~ compete in stability with all the fine long-established engram systems. In this regard I ~~[illegible]~~ like to picture . . .

(S)

. . . . insert S . . . .

insert →

Insert
p 212

Insert S

two factors at work in engram formation: first, a transient disturbance or shift of excitatory threshold that tends to recover within a half hour more or less, and, secondly, a metabolic-type factor that ~~to~~ constantly is at work and tends to ~~maintain and~~ reduplicate or freeze the status quo. A slower process, this latter has little effect over intervals of less than 20 minutes or so. The near perfect replication within the engram structure that is achieved in the metabolic turnover throughout a human lifetime is always a source of amazement and may be indicative of the kind of physico-chemical structure to look for in the engram. An alternative would be ~~mutational shifts~~ trace systems which, like the nucleic acid of the gene, are subject to little or no metabolic turnover.

p. 212 (cont'd) suggesting that most of the severed optic axons must succeed in reestablishing functional connections.

normal fish. ¶ Some of the corpus callosum work that Myers and I (J) have been doing shows that the memory trace system established with unilateral input is set up not only in one hemisphere, but that there is a duplicate set of traces set up in the opposite hemisphere via the corpus callosum. You can cut out the ~~whole trained hemisphere~~ cortex on the trained side, or section the callosum after training, and you find that the memory survives ~~effected through~~ in the opposite hemisphere.

Well, there are various other--

p. 212, line 33:

SPERRY: I think ~~that you can almost say categorically~~ it is fair to say that we know of no irrelevant or external agents that can wipe out the ~~the traces~~ engrams. Temperature changes, magnetic fields, concussion, electric currents, drugs and the like are ineffective. We have nothing as yet, excepting just the normal nerve impulses can put them in and possibly can wipe them out. (This latter remains a question, as already ~~mentioned~~ indicated, i.e., whether or not impulses can activly wipe out the ~~'temporary connections'~~ memory trace.)

insert (R) & (S) → One may wonder.....

~~Dr. Kornorski (xxxxx)~~ ¶ A modification of Pavlov's theory has been ~~prepared~~ proposed by ~~Dr.~~ Kornorski (K) in which he suggests that ~~each~~ stimul~~us~~i ha~~s~~ve both a gnostic, high-level, effect and a lower affective ~~motivational~~ component, and that the new connections are formed between the gnostic center of the conditioned stimulus and the affective center of the natural reflex. Dr. Liddell has mentioned that he thinks no good brain theory has been suggested to replace Pavlov's, or at least, nothing adequate. ~~and I suppose most of us will agree that there has been no really substantial substitute.~~ At most we have had some vague thinking about the nature and location of the new connections laid down between the conditioned stimulus and response centers; ~~at least,~~ to the effect that they must at least be more complex than the direct transcortical linkages proposed by Pavlov, that they probably involve subcortical centers,

p. 212 (cont'd)

and that possibly some kind of reverberatory activity is important in the earlier stages (L). ¶ Some years ago (M) I stuck my neck out to suggest that the conditioned reflex does not necessarily depend upon the establishment of any type of traces or connections between these two brain centers, but that the neural association between conditioned stimulus and response is a purely functional one and is effected in quite a different way ✓ which is probably too long a story to go into now. ———)

|—— Briefly, the suggestion is that the engrams support ~~traces of preceding conditioning trials are responsible for~~ the arousal of an 'expectancy' of what is coming in the conditioning situation. In instrumental conditioning, this leads to the establishment of a preparational facilatory set. The excitations of the conditioning ~~signal~~ stimulus, then, are routed ~~overflow~~ into the new pathways of the CR by the existing pattern of facilitation and inhibition imposed by this transient facilatory set. Within this scheme there is no need to search for 'connections' established between conditioned stimulus and response centers, as has been almost universally assumed, because there are none there. There is only an evanescent opening or facilitation of these (preexistent) pathways in thex contioning situation. ~~of already existing pathways~~ ~~leading to the conditioned response~~. The permanent traces that lead to arousal of the expactancy and preparatory set may be extremely complex and diffused and are tied not particularly to the specific CS, but to countless stimuli associated with the conditioning experience.

Well, this is probably ~~plenty~~ enough for now. ~~and~~ I am sure others can ~~perhaps~~ think of similar ~~add further~~ background material ~~of this sort~~ ~~that we would like to keep~~ and issues relevant to the brain mechanisms that it would well to have in the back of our minds ~~in~~ when we come to consider~~ing~~ the new data frkm the implanted electrode studies.

p. 219

line 15: SPERRY: Dr. Gantt, if you record ~~the~~ heart rate and respiratory rate, don't these appear in both instrumental and in classical conditioning, and don't they appear prior to the specific conditioned response ~~CR~~ such as salivation or leg flexion?

line 32: SPERRY: Yes, and I was thinking here that ~~of~~ these early visceral effects may ~~as~~ indicate ~~ing perhaps~~ a common basis for both types of conditioning. It may be that the classical is somewhat simpler than the instrumental, because, in the instrumental, the animal has to learn not only what to expect from the signal stimulus, but also what kind of reaction to make to best handle the situation; whereas under the conditions of classical conditioning, the animal needs only to learn what the signal stimulus brings ~~leads to~~ and the anticipatory ~~preparatory~~ ~~appropriate~~ response comes automatically ~~naturally~~. ~~with no further learning being necessary~~.

p. 222

line 10: SPERRY: Perhaps it is worth emphasizing that literally thousands of studies have been made since the first demonstration of the conditioned reflex in attempts to solve this seemingly simple phenomenon, and that the thing has turned out to be worse than a Chinese puzzle, the solution to which we are still not even close to a good half century later. In this short meeting, I suspect we can't ~~effectively~~ hope to achieve an effective encyclopedic coverage but will have to be ~~[illegible]~~ selective, trying to pick out those things that really bear on the brain problem, and trying especially to point up some of the more critical issues that ~~[illegible]~~ have come out of the work to date, on which the implanted electrode data may soon ~~bring new insight~~ shed new light.

p. 239

line 32-35: SPERRY: delete this (whole) passage.

p. 243

line 25 SPERRY: ~~Isn't~~ Is there ~~some~~ any chance that the motor stimulus is evoking a somatic sensation, ~~somewhere~~, perhaps a tingling of some sort in the paw or leg that is lifted?

line 29: SPERRY: So you may be dealing with two sensations in close succession, ~~which are~~ associated also ~~associated with an involuntary~~ with the afferent effects of raising of the leg.

p. 245

line 6: SPERRY: Probably ~~among the numerous older studies in the learning~~ of mental associations in man, one could find examples of associated sensations, images, and conditioned illusions and hallucinations. We also have demonstrations of sensory-sensory conditioning in animals. The technique here is to pair two stimuli repeatedly, then condition a response to

~~SPERRY: There have been demonstrations of 'sensory-sensory' or 'pre-sensory' conditioning in animals.~~

Same insert

(K)

(2) p245 l.6

insert p244 l.6

Sperry: There have been reports of "sensory-sensory" or so-called "sensory preconditioning" in animals. The technique, as I recall, is to pair repeatedly two stimuli, and then condition a response to the second one. Afterward it is found that the first of the paired stimuli, that never was used in conditioning the response, will, by itself, evoke the conditioned reflex. In man, as you know, there are numerous studies dealing with the acquisition of mental associations of various kinds some of which closely approach sensory-sensory conditioning. Also it has been shown that by

~~conditioned evocation by a with other sensory experience, sensation, imagery, illusion and even hallucination in suggestible or hypnotized subjects.~~

~~sensory illusions and even hallucinations can be evoked by a~~

conditioning procedures one can get a sensory stimulus to evoke sensory illusions and hallucinations ~~at presentation of the conditioning stimulus.~~

p. 248

line 18: SPERRY: This must mean, then, that the cerebellar stimulation you mentioned earlier ~~may~~ could have been evoking a sensation independently of ~~any~~ feed back from the forced movement.

p. 250

line 5: SPERRY: Dr. Doty, before we leave the subject, ~~of motivation,~~ another point comes to mind relating to motivation and its general role in conditioning: I believe that the so called "latent learning" which Dr. Olds referred to earlier is sometimes cited to indicate that motivation may not be necessary for the establishment of new linkages ~~by~~ in learning and conditioning. There is also a lot of seemingly unmotivated, "incidental" learning that is cited in the same connection, as is also sensory-sensory conditioning. As I recall it, there is one school of thought that claims that any two excitation processes occurring contiguously in the brain tend to become associated regardless of any reinforcing reward or motivational value, and another school that believes new linkages are not ~~formed~~ retained in the absence of some kind of reward which, of course, implies underlying drive and motivation.

replace by insert

~~The question is still open.~~ Also open is the related question of whether it is necessary in conditioning that the stimuli employed register centrally as sensation in subjective awareness. This, of course, ~~is a knotty~~ raises a knotty philosophical issue, but I suspect, as many maintain, it is not a pseudo- ~~or~~ nor an unimportant problem ~~as we get closer~~, especially now that we are getting closer and closer to the central brain mechanism.

"insert" (X) p. 250

~~I'm inclined to think that the positive and negative feedback controls~~

~~In any case it is possible that strengthening the positive and negative feedback systems~~

Motivation, operating via ~~[illegible]~~ high-level positive and negative feedback systems, the basic centers for which are being so nicely delineated in the self-stimulation methods of Dr. Olds and others, constantly directs behavior, unlearned as well as learned. Its obvious importance in conditioning may be only an indirect one with respect to ~~[illegible]~~ ~~the~~ formation and reactivation of the engrams, i.e. it may select favor ~~[illegible]~~ repetition and perseveration of adaptive contiguities as against ~~[illegible]~~ nonadaptive ones if you see what I mean. In any case the question is still wide open.

Also open is . . . . . .

p. 269

line 6: SPERRY: It would be an easy control to put in a piece of polyethylene sponge and stimulate it.

line 10: SPERRY: delete this remark.

line 23-35 SPERRY: delete this whole passage.

line 27: SPERRY: Did I understand you correctly to the effect that a locus in the caudate previously neutral was changed into an avoidance locus by conditioning procedure? How long did that alteration survive?

line 30÷33: Delete this whole passage.

p. 290

line 4-5: SPERRY: Delete this passage.

p. 301

line 14: SPERRY: I would object to that, Bob, but go ahead.

line 18: SPERRY: Yes, but not between the two response or stimulus points.

line 22: SPERRY: That's getting pretty safe, but I think I still object to possible connotations. But let's [redacted] go on.

p. 326

line 20÷29: SPERRY: Delete this whole passage.

line 31: SPERRY: Delete this remark.

line 35: SPERRY: Delete this remark.

p. 327

line 18: SPERRY: Would there be a heart-rate conditioning evident by this time, or a respiratory change?

line 22: SPERRY: Delete this remark.

p. 330

line 29 SPERRY: delete this remark.

p. 331

line 27: SPERRY: Does the normal monkey do any blinking with these flashing lights? Is there a wincing response under these conditions?

line 30: SPERRY: delete this remark.

line 33: SPERRY: delete this remark.

p. 332

? sp

line 1: SPERRY: I was not thinking of artefacts, but of some kind of central component of a protective flinching or blinking reaction. Is there no indication of such a ~~muscular~~ response?

line 6: SPERRY: I'm wondering about the source of such a rhythm whether it's a purely sensory central effect or involves a more complicated system with perhaps motor and peripheral components.

p. 332

line 10: SPERRY: The 3 to 12/sec ~~3-12 per second~~ rhythm.

p. 335

line 20-23: SPERRY: Delete this remark, ~~passage~~.

p. 367

line 17: SPERRY: Do you have any guess as to what system is mediating the repetitive response in this case?

p. 370

line 18: SPERRY: Is there any chance that there is some uncontrolled pairing with something like your reaching for a light switch, or something of the kind?

line 23: SPERRY: Completely isolated, and no consistent timing that the cat might anticipate?

p. 371

line 13: SPERRY: How did you define that difference between expectancy and conditioned response?

p. 413

line 14: SPERRY: I wish I could remember correctly how I got on that list (laughter); I think that it goes back to a pre-coffee-break presentation--

line 19: SPERRY: In ~~retrospect~~ thinking back, I believe I was concerned about the distinction that Dr. Olds was making between expectancy and conditioning. I think it's worth a further comment because some of us believe that the formation of an expectancy -- or should I say the neural correlate thereof -- is the basic factor in conditioning. The animal learns what to expect from the signal stimulus in the conditioning set-up, he perceives what follows what, and prepares to respond accordingly. This is important from the theoretical standpoint because it directs your thinking away from the almost universal assumption that the temporary connections, or engrams, must be laid down in some form between the conditioned stimulus center and the conditioned response center. This is why I objected yesterday to the statement, even in qualified form, that ~~this~~ some such connection is what we are looking for.

p. 417

line 33-35: SPERRY: Delete this remark.

Keep these on separate page

References (cont)

D.5) –(see ~~on~~ next page)

E.) Lashley, K. S.: In search of the engram. in Physiological Mechanisms in Animal Behavior. Symp. Soc. Exp. Biol. IV 1950, (p. 454).

F) Humphreys, L. G.: The effect of random alternation of reinforcement ~~and~~ on the acquisition and extinction of conditioned eyelid reactions. J. exp. Psychol, 25, 141, (1939

G) Ellson, D. G.: Successive extinctions of a bar-pressing response in rats. J. gener. Psychol. 23, 283, (1940).

H) L. E. Cole – already cited (see A).

I ~~H~~) Sperry, R. W.: Regulative factors in the orderly growth ~~patterning~~ of neural circuits. Growth Symp. vol X; 63 (1951).

(J) Myers, R. E. and ~~R. W.~~ Sperry, R. W.: Contralateral mnemonic effects with ipsilateral sensory inflow. Fed. Proc. 15, p.? (1956)

(K) Konorski, J.: Mechanisms of learning. in Physiological Mechanisms of Behavior. Symp. Soc. Exp. Biol. IV, 409, (1950).

References (cont)

(L) Hebb, D. O. The Organization of Behavior, John Wiley & Sons, Inc., London (1949).

(M) Sperry, R. W.: On the neural basis of the conditioned response. Brit. J. anim. Behav.: 3, 41, (1955).

(D.5) Rensch, B. and Franziskeb, L.: Lang andauernde bedingte Reflexe bei Rückenmarksfröschen. Zeit. f. vergl. Physiol. 36, 318, (1954).

File in + have typed

References

1 carbon

N - Stamm, J. S. and Sperry, R. W.: Function of corpus callosum in contralateral transfer of somesthetic discrimination in cats. J. comp. physiol. Psych. 50, 138. (1957).

O - Sperry, R. W.: Corpus callosum and interhemispheric transfer in the monkey (Macaca mulatta). Anat. Rec. 131 (in press) (1958).

P - Sperry, R. W., Stamm, J. S. and Miner, N. M.: Relearning tests for interocular transfer following division of optic chiasma and corpus callosum. J. comp. physiol. Psychol. 49, 529, (1956).

Q - Sperry, R. W.: Experiments on perceptual integration in animals. Psychiat. Res. Rep. 6, 151, (1956)

R. - Myers, R. E.: Function of corpus callosum in interocular transfer. Brain, 79, 358 (

S. Myers, R. E. Interhemispheric communication through corpus callosum: limitations under conflict. (in press).

T. Myers, R. E. and Sperry, R. W.: Interhemispheric communication ~~through~~ the corpus callosum. Mnemonic carryover between the hemispheres. Arch. Neurol. Psychiat. (in

Captions for Figures

Figure 1. Photographs of a cat split-brain preparation showing (A) cortical remnant in right hemisphere (A) that mediated retention and new learning of somesthetic discriminations performed by left paw. Subsequent reciprocal lesion made on left, shown better in lateral view (B), abolished all but a bare trace of discriminatory performance with right paw without impairing that of left paw. J. Neurophysiol. (in press).

Figure 2. Sketch illustrating use of split brain in monkey to study perceptual integration between vision and somesthesis.