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.

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. because 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, l. 29

SPERRY: Delete.

p. 464, l. 31

SPERRY: Delete (delete whole passage l. 27-32?)
center of the natural reflex. Dr. Liddell has mentioned that no good brain theory has been suggested to replace Pavlov's, or at least, nothing adequate. At most we have had some vague thinking about the possible nature and location of the new connections laid down between the conditioned stimulus and response centers; to the fact that they must be more complex than the direct transcortical linkages proposed by Pavlov, that they may involve subcortical centers, and that possibly some kind of reverberatory activity is important in the earlier stages.

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Some years ago 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 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 a perception to the arousal of an 'expectancy' of what is coming in the conditioning situation. In instrumental conditioning, this leads to the establishment of a preparatory facilitatory set, the excitations of the conditioning stimulus then are routed into the new pathways of the CR upon the existing pattern of facilitation and inhibition imposed by the transient facilitatory 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 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 shortly.

R. W. Sperry

p. 150
2.6 SPERRY: Delete this whole p.----
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 significant development in brain theory of conditioning since Pavlov’s time?

p. 167, line 17:
SPERRY: On thinking back.

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. 176, line 17:
SPERRY: Has anyone tried to establish these low-level, visceral-visceral conditioned reflexes in decorticate animals?
SPERRY: There are reports of spinal conditioning in the frog by Rensch's laboratory that seem to be pretty good in chronic spinal. They pair a strong and a weak cutaneous stimulus. The stronger stimulus, to the flank, say, dominates during the conditioning trials and inhibits the 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 evokes the flank wiping 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 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?
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 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, pretty much eliminates that concept. It is difficult to reconcile with any such. 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.

Now that we are

I too have had the feeling that in approaching the new implanted electrode work, it would be helpful, particularly for those of us not working on conditioning, to have a more brief description of those conditioning phenomena that have seemed particularly relevant to brain theory. I can
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. Apparently, it goes perfectly well in the absence of any motor response. The motor response has been eliminated, 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.

SPERRY: In any case, there have been experiments in which ablation of the motor cortex has failed to abolish learned responses. 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 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 that with an equal number of trials in the conditioning procedure, aperiodic, rather than regular reinforcement, produces a CR that is much more difficult to extinguish than is the CR formed with regular reinforcement. According to most of the physiological explanations, including that of Pavlov, you would expect to get a much stronger connection between the brain centers involved if you pair the conditioned and unconditioned stimuli on every trial. The effect of alternate reinforcement and extinction has already been mentioned. If you establish a conditioned response, then extinguish it thoroughly then reestablish it, and then extinguish again, it has
p. 209, line 22 (cont'd)

after so many repetitions of this, the learned reflex can be reestablished with a single trial. This too has important implications for the underlying brain process.

Just the phenomenon of the delayed reflex 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, and 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 some kind of a built-in tendency to perceive what-follows-what, 'what-leads-to-what'. Appropriate中枢 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 one problem in the acquisition of the conditioned reflex, and another associated with its prolonged retention. Generally it is not easy to distinguish 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, if it
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, within 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 problem primarily of dynamic reorganization. This is what establishes the new connection. In time the reorganization is consolidated and lasting tissue changes come to effect retention.

For convenience in dealing with the brain changes, we can separate these two phases of the process phenomenon: the reorganization process, and then the tissue changes for retention, by which the reorganization is consolidated and retained.

The effect of electroconvulsive shock is of interest in that it is the fact 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 eradicate the more permanent trace 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 illustrating the dynamic reorganization is the experiments on human subjects. It has been shown that under the usual conditions, it takes a conditioned response that required some twelve trials to establish was performed
on the very first trial with no training when the subjects are given
a full understanding of what to expect in the conditioning procedure.

p. 211, line 21:
SPERRY: Yes, this 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 whether this is true.

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

In this connection Dr. Atta in our laboratory has recently confirmed
that a visual discrimination can be retained after complete section
and regeneration of the optic nerve, showing that the memory traces
are not rigidly but directly connected to the sensory input chan-
nels. There probably is a certain amount of
reshuffling of optic fiber connections in
the brain as a result of regeneration. We think the fibers get back
pretty close to the same cells, but probably 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 after regeneration in its original
form. The restored visual acuity also approximates closely that of the
other visual input.

I believe, however, the existence of another dimension of
specificity among the optic fibers associated with color, presumably
superimposed upon the topological specificity associated with
directionality
An animal will easily and spontaneously substitute for the conditioned response a quite different response if the situation changes, regardless if it, or if the goal is perceived to be more readily achieved thereby. Continuous motor readjustment of this kind is essential in the learning of new motor skills.

I don't recall anyone having ever seen the phenomenon of motor equivalence in instrumental conditioning. It is difficult to account for with any theory that postulates the weaving of connections between CS and CR centers. The observation in this case is that an animal will
In this connection one wonders why the impulses generated by electroconvulsive therapy shock are as effective in establishing traces as are impulses generated in organized activity. I don't know if repeated frequently enough, ECS treatment does gradually wear blanknesses and confusion into the brain, which in time begin to compete in stability with the long-established engram systems. In this regard I like to picture...
Two factors at work in engram formation: first, a transient shift of excitatory threshold that tends to recur more or less within a half hour and, secondly, a metabolic factor that is constantly at work and tends to maintain and replicate the status quo. A process which has little effect upon the organism on intervals of less than 20 minutes or so. The process of replication within the engram structure that is achieved in the metabolic turnover throughout a human lifetime is always a source of enhancement and may be indicative of the kind of change in the physical-chemical structure to look for in the engram. An alternative would be trace systems like the nucleic acids of the gene, are subject to little or no metabolic turnover.
p. 212 (cont'd) That most of the severed optic axons must succeed in re-establishing functional connections.

Some of the corpus callosum work that Myers and I (T) have been doing shows that the memory trace system is set up not only in one hemisphere, but that there is a duplicate set of traces set up via the corpus callosum center on the trained side, or in the opposite hemisphere. You can cut out the whole trained hemisphere and you find that the memory survives across through the opposite hemisphere.

Well, there are various other—

p. 212, line 33:

SPERRY: I think that one of the reasons is that we know of no irrelevant or external agent that can wipe out these memories. Temperature changes, magnetic fields, electric currents, drugs—none of these have nothing as yet, excepting just the normal nerve impulse can put them in, and possibly can wipe them out. (This latter remains a question, i.e., whether or not impulses can actively wipe out the memory trace. One may wonder…)

A modification of Pavlov's theory has been proposed by Kornorski (K), in which he suggests that a stimulus had both a gnostic, high-level, effect and an 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. We have had some vague thinking about the connections between the conditioned stimulus and response centers; it is to the effect that they must be more complex than the direct transcortical linkages proposed by Pavlov, that they probably involve subcortical centers,
and that possibly reverberatory activity is important in the earlier stages. 

Some years ago I stuck my neck out to suggest that the conditioned reflex does not depend upon the establishment of any type of traces or connections between the two brain centers, but that the association is a purely functional one and is effected in quite a different way, which is too long a story to go into now.

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Briefly, the suggestion is that the traces of preceding conditioning are responsible for the arousal of an 'expectancy' of what is coming. In instrumental conditioning, this leads to the establishment of a preparational facilitatory set. The excitations of the stimulus, then, are routed into the pathways of the CR by the existing pattern of facilitation and inhibition imposed by this transient facilitatory set. 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 of these (presumably) pathways evanescent opening or facilitation in the conditioning situation, already existing pathways. The permanent traces that lead to arousal of the expectancy 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 enough for now, and others can, perhaps, and issues relevant to the brain mechanisms that I want to keep well to have in the back of our minds when we come to background material that would like to keep in the back of our minds considering the new data from the implanted electrode studies.
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 such as salivation or leg flexion?

line 32: SPERRY: Yes, and I was thinking here that these early visceral effects may indicate 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 brings and the appropriate response comes automatically 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 achieve an exhaustive coverage but will have to be selective, trying to pick out those things that really bear on the brain problem, and especially the point up some of the more critical issues that which the implanted electrode data may soon bring new insight.

p. 239

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

p. 243

line 25: SPERRY: Isn't there any chance that the motor stimulus is evoking a somatic sensation, 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 also raising of the leg.

p. 245

line 6: SPERRY: There have been demonstrations of 'sensory-sensory'
condition a response to 'pre-sensory' conditioning in animals.
There have been reports of "sensory-sensory" or so-called sensory preconditioning "maneuvers." The technique 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 paired in conditioning, the response, will, by itself, evoke the conditioned reflex. In many cases, there are numerous studies dealing with the acquisition of mental associations of various kinds among which classical sensory conditioning also has been shown that by association, phasic sensation by itself with other sensory experience, vision, hearing, taste, hallucinations in susceptible or hypnagogic subjects, sensory illusions and even hallucinations can be evoked by a conditioning procedure. We can then pair a stimulus to evoke sensory illusions and hallucinations at presentation of the conditioned stimulus.
SPERRY: This must mean, then, that the cerebellar stimulation you mentioned earlier have been evoking a sensation independently of feedback from the forced movement.

SPERRY: Dr. Doty, before we leave the subject, 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 in learning and conditioning. There is also a lot of seemingly unmotivated, "incidental" learning that is cited in the same connection. 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 in the absence of some kind of reward which, of course, implies motivation. 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 problem, but I suspect, not a pseudo-problem to the central mechanism.
I'm inclined to think that the positive and negative feedback controls

In any case it is possible that modulation of the positive and negative feedback systems

Motivation, operating via high-level positive and negative feedback systems, the basic centers for which are being so nicely delineated in the self-stimulated 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 formation and reactivation of the engrams, i.e. if may select favor repetition and perseverance of adaptive contingencies - nonadaptive ones if you see what I mean. In any case the question is still wide open.

Also open is . . . . . . . .
line 6: SPERRY: It would be easy 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.
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. Let's 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.
SPERRY: delete this remark.

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

SPERRY: delete this remark.

SPERRY: delete this remark.

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 sensor response?

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.
line 10: SPERRY: The second rhythm.

line 20-23: SPERRY: Delete this remark, please.

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

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?

line 13: SPERRY: How did you define that difference between expectancy and conditioned response?
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 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 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 is what we are looking for.

p. 417

line 33-35: SPERRY: Delete this remark.
References (cont)

   in Physiological Mechanisms in Animal Behavior

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

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H) H. E. Cole - already cited (see A).

I) Sperry, R.W.: Regulative factors in the order of growth of neural circuits. Growth

J) Myers, R. E. and R. W. Sperry, R.W.: Correlation of mnemonic effects with ipsilateral sensory

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

Figure 1. Photographs of a split-brain preparation showing cortical remnants in right hemisphere (A) that mediated retention and new learning of somesthetic discriminations performed by left paw. Subsequent reciprocal lesion on left, should better in lateral view (B), abolished all traces of discriminatory performance with right paw, J. morphophys. (in press).

without impairing that of left paw

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