UNIVERSITY OF CALIFORNIA, LOS ANGELES

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO

SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF ANATOMY
SCHOOL OF MEDICINE
THE CENTER FOR THE HEALTH SCIENCES
LOS ANGELES, CALIFORNIA 90024

May 14, 1964

Dr. Leo Szilard
The Salk Institute for
Biological Studies
P. O. Box 9499
San Diego, California

Dear Dr. Szilard:

Thank you for your kindness in sending me a copy of your paper on "Memory and Recall."

I found it quite an exciting paper to read, particularly if one divorces it completely from any biological context. I think it is necessary to bear in mind a quite basic philosophical premise that there are models that concern themselves with the question as to whether the model as such would work effectively to achieve its purpose, and those models which have some realistic relationship with what does in fact occur in the brain.

The pursuit of the first type is an entirely commendable activity, and through it we may ultimately come to some understanding of the many ways in which storage and recall of information might be achieved in complex systems. On the other hand, if we are seriously concerned with the brain as a brain and with those functions which uniquely characterize in the storage and recall of information, it seems to me necessary that we take some account of those structural and functional relationships which may uniquely determine this capability.

As I was at pains to try and point out in my discussion at the La Jolla meeting, the question of modes of interaction between neurons as inherently determining the basis of information storage requires that we take some account of their physical interrelationship, as well as the question of propagated activity across synaptic junctions. Despite the wealth of such junctions in tissue such as the spinal cord, the apparent inability of the neurons in the spinal cord to store information as we usually conceive it leaves me with grave doubts about schemes which seek to explain memory without regard to the requirement for a minimal interacting domain of neurons, having a certain population size in a specified physical configuration

to one another. As I indicated at the meeting, it would appear that this physical configuration is one of overlapping dendritic fields, as was first suggested by Sholl in 1955.

Your notion of "congenitally-determined" neurons and "memory" neurons is interesting in the concept that you propose for transprinting. A time-gated arrangement of this type was proposed 15 years ago by Grey Walter. However, the question of congenital-determination probably has only very limited application in the mammalian brain. It is probably of least importance in the so-called associative systems that are the ones primarily concerned with memory functions. So much work has been done on reorganization of activity in brain systems following damage or removal that we must necessarily take account of the plasticity of organization at these higher levels in reshaping patterns of activity in the face of some peremptory disruption. Moreover, our studies and those of others on the behavior of single nerve cells during the Pavlovian conditioning process does not suggest in any way that the onset of a changed firing pattern during Pavlovian conditioning occurs suddenly in an "all or none" fashion. Rather, it is a gradually developing series of phenomena over a period of 20 minutes or more, involving a minimum of 50 to 100 repetitions. In particular, as I pointed out at the meeting, the conditional changes in firing pattern so induced may be repeatedly extinguished and reinduced, with the appearance of even more complex changes during repeated extinctions of the response. Thus, the notion of memory at the single cell level as involving some single "all or none" process finds little or no support from the evidence currently available.

Much of the work that I presented at the meeting that is germane to your topic remains in manuscript form, but I hope to have the privilege of sending reprints to you when it becomes available. Meanwhile, I enclose a few recent reprints of our studies which relate to the uniqueness of organization of the brain as we have discerned it in relation to the memory process.

I very much appreciate your kindness in sending this preprint to me.

Sincerely yours,

Ross Adey

W. Ross Adey

WRA/gc

Enclosures

15 May, 1964

Dr. W. Ross Adey
Department of Anatomy
School of Medicine
University of California
Los Angeles, Calif. 90024

Dear Dr. Adey:

Many thanks for your letter of May 14. I wonder whether you could supply me with a reference to the "time-gated arrangement proposed fifteen years ago by Grey Walter" or whether you could send me any reprints of Grey Walter which I could photostat and return to you.

I am quite familiar with the Pavlovian experiments which are described in Pavlov's book published in 1927 and I seem to have no difficulty in explaining this on the basis of a neuron network similar to that given in Part 1 of my paper but I am not familiar with any of the later experiments.

On my next visit to Los Angeles I would very much like to have a chat with you on this and related matters and I shall take the liberty to call you ahead of time, from La Jolla, to see if we can make a date which is convenient to you.

Sincerely yours,

Leo Szilard

LS:jm

file C.N.S

UNIVERSITY OF CALIFORNIA, LOS ANGELES

BERKELEY • DAVIS • IRVINE • LOS ANGELES • RIVERSIDE • SAN DIEGO • SAN FRANCISCO

SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF ANATOMY
SCHOOL OF MEDICINE
THE CENTER FOR THE HEALTH SCIENCES
LOS ANGELES, CALIFORNIA 90024

May 22, 1964

Dr. L. Szilard
The Salk Institute for
Biological Studies
P. O. Box 9499
San Diego, California 92109

Dear Dr. Szilard:

Thank you for your letter of May 15, inquiring about Grey Walter's hypothesis on temporal coincidence in learning.

I first learned of this material during a visit to his laboratory in 1950, when he had just presented it at a meeting in Paris. I have only my original notes, and no reprints. However, I think I have located the reference in our library, but not the actual material. It is in a volume edited by M. Monnier, entitled "L'organisation des fonctions psychiques," 103 pp. Dunod à Paris et Edition du Griffon à Neuchâtel, 1951. This concept was the basis of the various "tortoises" that he constructed and to which he refers in his book, "The Living Brain."

I would like very much to have the privilege of personal discussions with you on this topic, and may, indeed, be in the La Jolla area before long. If so, I shall call ahead to see if it would be convenient to see you.

I spent May 18 at NIH in the company of Bob Livingston and bring you and your wife warmest greetings.

Sincerely,

Ross Adey

W. Ross Adey

WRA/gc