

Preface

The idea that circuits might be able to explain brain function never really occurred to me until once when, as a Princeton undergraduate, I heard a lecture about a Cornell psychologist, Frank Rosenblatt, and the machine he had designed, the *Perceptron*, which could “learn” to recognize letters of the alphabet. The lecture planted a seed, but it took very long before anything grew from it.

I went to Cornell after graduating from Princeton, to work on a PhD in theoretical physics, and made occasional visits to Rosenblatt’s lab. I kept telling him how I didn’t think the brain worked the way he described, and he countered by pointing to the large number of “theories” I could choose from. Of course I had no idea how the brain worked, but in the process I started reading books on it.

It was hard to know where to start. Physiology, anatomy, psychology, neurology, computer science – these were all subjects directly relevant to what I needed to know, not to mention that data and facts about the human mind are all around us; and observations too common to require laboratory research are not automatically unimportant.

I finished my PhD work (on solid-state plasma oscillations known as *helicons*), and proceeded to a job with United Aircraft (now United Technologies) in Hartford, as a physicist under a contract which permitted me to spend one third of my time on work I chose, which in my case would be brain theory.

From Hartford I often drove up to MIT, where McCulloch had his office, cluttered with papers, and Lettvin his lab, with the name tag “J. Y. Lettvin, experimental epistemology.” They were both still active, and gave generously of their time. At United Aircraft I wrote my first brain theory paper, on ignitable neuron groups (Legéndy, 1967). Influenced by the approach of McCulloch, Rosenblatt, and von Neumann, the 1967 paper attempted to explore the duality between subjective perception and the facts of connectivity, and describe a set of “networks with circles” which are more reliable than their individual elements.

The proper career path for me at this point would probably have been to enroll in a graduate school again and study under some established neuroscientist, but by this time I have become aware that there was nobody able to teach me what I needed to know. I was navigating uncharted waters.

My good luck steered me to a small Westinghouse research group, located right on “Tech Square,” a block of research buildings across the street from MIT, where

I was offered a job doing pure research in brain theory, justified as potentially leading to computing machines better suited to pattern recognition than digital computers. Tech Square was the world's greatest place to learn about brains. I often spoke to McCulloch, got to know Minsky and Papert, and, on the other side of the river, at Harvard Medical School, got to visit the labs of Hubel, Wiesel, and Palay.

While at Westinghouse, I wrote down many of the principles which underlie the present book; among them were the need to formulate communication inside the brain in terms of "surprising" events of firing, the conceptual linkage between them and "local knowledge," and the idea that neuron groups representing objects can transmit "syntactic" relations between the objects through prearranged relative timing of their outputs (Legédy, 1970). I also wrote down the idea of "reaching," and some methods the brain uses to achieve it, such as the "trick of retinotopic mapping" and the "trick of small connective fields" (Remarks on the Brain, unpublished internal report, 1970).

After my Westinghouse money ran out, I took a postdoctoral position in Italy, at the lab of Caianiello (at the CNR Laboratory of Cybernetics at Arco Felice), then another one in Germany (at the University of Tübingen), a few minutes away from the Max Planck Institute of Biological Cybernetics, where Valentino Braitenberg had his group. I spent many good hours at Braitenberg's office looking at Golgi slides through his microscope.

I still had much to learn. For one thing, I had felt since my conversations with Hubel that my education would remain incomplete until learned to do experiments of my own. My years in electrical engineering (which I had studied at Princeton) steered me toward electrophysiology, and an opportunity to learn the techniques soon arose at Otto Creutzfeldt's lab in Göttingen, where I had a chance to join a project with Tadaharu Tsumoto on cortico-geniculate correlations. At Göttingen I went on to spend a very busy one and a half years, and wrote, among other things, a program to make "Poisson surprise scans" of some spike trains I recorded. By the time I returned to the New York City, I knew enough to take postdoctoral jobs as a neurophysiologist, first with Alden Spencer at Columbia University, then with Herb Vaughan at Albert Einstein College of Medicine.

But my career in experimental brain research was short-lived, because after a while I lost my NIH funding and had to take jobs in the aerospace and computer industry. It seemed that my brain research days had come to an end. Except that around this time my good luck once again intervened.

Wayne Wicklegren, formerly of MIT and the University of Oregon, moved to New York and joined his wife Norma Graham at Columbia University. I knew both Wayne and Norma from the literature, as they both knew me; in particular Wayne, encouragingly, had a lively interest in my 1967 paper. We talked about the brain on many afternoons for several years; and by the time we stopped, I was doing brain research once again, this time not experiments but theory. Along with my wife, Annemarie, who patiently stuck it out with me throughout, Wayne deserves my deepest thanks for the push he gave me when I most needed it.

During working hours I continued my computer jobs, but before and after work I started filling notebooks with my thoughts on vision; then after retiring I started typing them into a computer, until this book came together.

The book you see here does not deal with the whole brain, only with vision, and within that subject only with one class of large image-determined circuits: the *contour strings*. My original intent was to include another class of large circuits, the “color pools,” and I had also hoped to extend the discussion beyond the V1 area of visual cortex, to V2, where the first version of a “stable” cortical image (which does not move around with the retinal image) promises to arise.

However, preliminary work convinced me that both of the other subjects, the *color pools* and the *stable image*, were hugely more complex than the *contour strings* and I decided to leave them out, at least for now.

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