

WARREN MCCULLOCH
A PERSONAL ACCOUNT

A. A. Verveen

Blue eyes with an ever present humorous twinkle in an open face framed with grey hair and a gray goatee, a slim and sinewy build with a lithely moving body, behind him the blackboards quickly covered with triangles, dots and crosses, a talk, spiced with reminiscences about many people and occurrences of his far flung travels, allusions and references to poets, scientists and philosophers of the past, all this conveying a strong sense of historicity; such is the still vivid impression Warren made on me in Amsterdam, back in 1958, during one of his many trips to the Netherlands.

I soon learned that his knowledge of the Dutch scientific scene equalled, even surpassed, that of many Dutchmen, including myself.

I had just rediscovered the random response of nerve fibers to threshold stimulation. Fascinated by irregularities in nature I had planned a closer investigation into this phenomenon. Two days before, in Amsterdam, I had presented my maiden speech on the subject, a ten-minute talk in a foreign language, English, at an international meeting on brain structure and function. This was before an audience among whom were famous people like Eccles (who, it became apparent, did not care), Lorente de No (who did not believe), and Warren McCulloch (who did believe me). After the meeting Henk van der Tweel invited me to come along and talk with Warren. Impressed as I was by him, McCulloch quickly put me at ease, welcoming me with the remark that now he finally had found a fellow who put a firm experimental basis under his theory of neuronal unreliability. This remained a strong theoretic-

cal interest of McCulloch's throughout the rest of his life. This was essential for me at that moment and also for many years, since for the whole following decade he remained the only scientist outside the Netherlands who really did believe in my work. But for his encouragement I would have given up scientific research long ago, to start working in clinical neurology and neurosurgery.

At that first meeting I asked him a question, which I had pondered for some time: Did he know of a biophysicist interested in noise who might model a noisy axon and look for the fluctuations in its response to stimulation? I was in need of such cooperation for I had come to the conclusion that fruitful work could only be done by cooperation, in "teamwork," as the present terminology is. I also soon learned that the only basis for good teamwork lay in the freely evoked interest in the problem of the potential participants. McCulloch's answer came immediately. Oh yes, he knew one, living close by, within a distance of less than thirty miles, in Utrecht: Marinus ten Hoopen, who formerly had worked in his lab at M.I.T. And there I stood astonished at the answer that came to me from the other side of the ocean! His suggestion was quickly followed up and resulted in a fruitful period of collaboration between Marinus and myself. This resulted later in a further quest, for fluctuating membrane potentials, for which a new team was needed.

When McCulloch received my thesis on *Fluctuation in Excitability* he invited me to come to Cambridge, Mass. to learn to know other scientists, to see the country and the American way of life and then - a "must" - to go back to Holland to work in my own country. This was a typical attitude of McCulloch's. I came, in 1962, and I went back, although I would have stayed had I met a physicist ready to work cooperatively on membrane noise measurements which McCulloch was actively interested in promoting in the U.S. But I could not find such a person at that time, although I travelled all over the country and visited several well known laboratories.

Dinner at night, with everyone seated around the large oaken table in the kitchen, was a lively affair. There was never a dull

evening in the house in Cambridge nor later at the farm in Old Lyme, in Connecticut.

McCulloch was not uncritical, he could become vehement when he saw bad work. Publications with results that he considered to be doctored were scathingly commented upon. He went out of his way to warn us time and again about such writers, and he was quite right in this we learned.

Until the very end of his life in 1969, McCulloch actively promoted my work. He did not live to see the general interest in research in membrane noise surge at the end of the sixth decade and spread in the seventies. But I am convinced that all along he foresaw that this would happen.

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