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WARREN AND WALTER

Jerome Y. Lettvin

Warren McCulloch and Walter Pitts wrote the *Logical Calculus* [51] (35) in '42-'43. It is, I believe, the first and so far the only theory proposed for generating a mental-like process from a nervous-like mechanism. Together with an earlier paper by Turing on the Universal Automaton (1937), it became foundational to the new art of computing by machine. Its impact was strongest on those who later began the research called Artificial Intelligence. I want to consider two contexts of the work, one personal, the other in the realm of ideas.

Walter Pitts and I became friends in 1938-1939 at the University of Chicago. Walter was then 15 years old, I was 18. He had left his family and never spoke to them again in our succeeding 30-odd years of friendship. It was already clear that he had broad genius. He had been working in logic with Carnap, who regarded him with delighted astonishment, as did Professor Rashevsky, head of the Department of Mathematical Biophysics, who was conducting a series of lively seminars which we attended now and then. Walter had no money and lived on what he could glean from student employment at the University of Chicago. That was somewhat complicated by the fact that he hadn't registered as a student, for he lacked a high school diploma. But he was not unknown to the faculty. Once he completely demoralized a class in a science survey course by showing up for a final examination, marking his answers on multiple choice after openly flipping a coin for each question, and then getting the highest grade. In 1941 Warren McCulloch came to the University of Illinois Medical School as head of the laboratory of the Neuropsychiatric Institute. Dr. Gerhardt von Bonin, my professor of anatomy, took me over to see him. McCulloch had the most piercing eyes I had ever met. I was fascinated by him and his influence led me to immerse myself in the study of the nervous system.

In the winter of 1941 McCulloch began attending Rashevksy's fortnightly seminars.

I had not lost touch with Pitts, but the pressures of medical school cut down on our meetings. However, I went with McCulloch to one of these seminars and introduced Pitts to him. As a result, by 1943, Walter and Warren had conceived and completed the *Logical Calculus*.

Pitts was married to abstract thought. Once, Pitts told us that when he was twelve years old he was chased by some bullies into a public library, where he hid in the stacks. There he picked up Russell and Whitehead's Principia Mathematica and could not put it down. For the next week he lived in the library from opening to closing time, going through all three volumes. It seemed to him then that logic was magic, and if he could master that magic and practice it, the whole world would be in his hegemony - he would be Merlin. But to do this one had to do away with self. Ego must never enter, but only Reason. And at that moment of revelation he committed ontological suicide. That is the peculiar truth about Pitts, whom all of us loved and protected. We never knew anything about his family or his feelings about us. He died mysterious, sad and remote, and not once did I find out, or even want to find out more about how he felt or what he hoped. To be interested in him as a person was to lose him as a friend. Moreover, if a question were asked about anything whatever - history, literature, mathematics, language, any subject at all, even empirics such as systematic botany or anatomy, out would come an astonishing torrent, not of disconnected bits and pieces of knowledge, but an integral whole, a corpus, an organized handbook with footnotes and index. He was the very embodiment of mind, and could out-think and out-analyze all the rest of us.

I do not know who contributed what to the *Logical Calculus* (c.f. McCulloch's own account). I did not understand it at the time, although I helped draw the figures for it and for that other great essay, *How We Know Universals* [76] (59). But the full generality of what had been done did not hit me until many years later. I recall several distinct negatives: neither Warren nor Walter knew of Turing's paper of 1937. I believe Pitts learned of it first in

1944 when he joined Wiener at M.I.T. Neither of them knew in 1942-1943 about Julian Bigelow's and John von Neumann's work on the digital computing machine. And, most important, neither of them connected what they had done with Leibnitz' ideas. That last came later, in the 1950's, after we had all moved to M.I.T. From their viewpoint the paper was completely original and novel, having its antecedents in Boole and in Russell, and its purpose was to understand how brain could so operate as to be the mechanism for mental process.

In 1943 I left Chicago for a neurological internship in Boston. My friend and co-intern there, Alden Raisbeck, introduced me to his distant relative, Norbert Wiener, whose young protege, Paley had just been killed in a mountaineering accident. I described Walter to him, and he professed disbelief. So Warren and I got Walter on a train to Boston. Walter's meeting with Wiener at M.I.T. was characteristic and beautiful. Wiener didn't greet us as we entered but said, "I want to show you something interesting about the ergodic theorem." We followed him to the blackboard. After about five minutes Walter interrupted with an objection. The board extended over two sides of the small classroom. By the end of the hour they were deeply immersed on the second wall. I left, because there was no question about what had happened.

After the war, Pitts and Wiener persuaded me to register as a special student in mathematics at M.I.T. I saw both of them often. In this period information theory was a new and fascinating topic and Wiener issued his commanding book on cybernetics. Pitts, who had joined Wiener at M.I.T. in 1943, was much taken up with both notions, and he and Oliver Selfridge boiled steadily around Wiener with ideas and plans.

But then Pitts was snapped up during the war by the Kellex Corp., i.e the Manhattan arm of the atom-bomb project, and he worked there for two years. On returning to Wiener, he began to look with great interest at large, randomly connected nets in two-space and three-space, which he proposed to treat by some novel methods of his own devising. I did not understand what he was doing when he later showed me the work, but what he had demonstrated was that under few and very loose constraints on such nets, any distributed input at one edge or surface could be mapped by a distributed output at another edge of surface, but not in a simple way. State histories on the input would be related to state histories on the output and, when the input history stopped changing the output eventually relaxed in a stationary time series. Some flavor of what he had found is reflected in the experiment that he tried to get me to do in 1951. We would undercut a slab of cortex, leaving pial circulation intact but severing all connections of the slab (as DeLisle Burns had done). Then, with many stimulating electrodes at one edge of the slab, and many recording electrodes at the other edge, we would get the function that related the running power spectrum of the recorded outputs against that of the known inputs. The experiment is, clearly, both technically difficult and a computational nightmare. He assured me that if it could be done physiologically the results would, in fact, give a meaningful measure of the orders of connectivity within the slab.

I mention this only to show that between 1944 and 1951 Walter had shifted his attention away from logic and toward problems of the sort that Wiener handled.

McCulloch, after publication of those two remarkable papers, the *Logical Calculus* and the *Universals*, also shifted his interest. Up to that time he had been an experimentalist, as brilliant in interpretation as he was skillful in performance. His great maps of the cortex in *The Precentral Motor Cortex*, still some of the best and most reliable studies ever done on cortico-cortical connections, have suffered a disuse that is undeserved. I find them reliable and interesting still, and consider most of the putative physiological criticism as ill-informed and baseless. Strychnine neuronography has been succeeded by newer and more highly resolving anatomical methods so that, in a way, the arguments for it do not matter now. McCulloch had a proper eye for phenomena and could spot a systematic error or artifact with speed and precision. He knew his apparatus. Furthermore, as his works show, he was game for any technical challenge. But by 1948 he was, dare I say it, bored with the laboratory. He had been converted by his own work into a searcher after theory.

The *Logical Calculus*, McCulloch knew, was not even a caricature of any existing nervous process. Indeed he made that very clear at the time of writing. But is was a possible and useful assembly of axiomatized neurons, and that seemed to him a far greater accomplishment than a true description of any definitely known neuronal circuit (of which none then existed). Similarly, the *Universals* did not claim to be an actual account of operations in a special sensory cortex, but rather a possible account. This and that could be done with components very like neurons. There were some who took the work literally and went searching for the circuits in the brain. Warren tolerantly felt that it was a good thing, because then one might find what such real circuits might be and devise a model for them.

But sooner or later the message of the two papers was offset by his years of experience with clinic and laboratory. Sensitive as he was to nuance, remote causation and the expected unexpected, this dry determinism of logic needed, it seemed to him, not so much softening as the introduction of contingency. So he began the quest for a new logic - something like the Holy Grail; one had no idea what it might be, but would recognize it on sight. He played with three-valued logic, proposed a probabilistic logic, spent whole nights for weeks on end with Venn diagrams and other ideograms, for, as everyone knew, the essence of mathematizing lay in the notation (101). I, who have a kind of agnosia with respect to logic, watched with more hope than understanding. This is not meant ironically, for there are several things that people outside research don't grasp. First is that there are many more mistakes than truths, and it often seems that the only way of finding one's way is through a maze of all possible errors. In retrospect, the straight path is so clear that anyone can see it, but by then the new highway is clogged and developers have taken over the surrounding land. Second is that every research man has a private language that he alone understands, and forming that language takes precedence over any translation into the public tongue. So I watched the progress with more curiosity than confidence, for I could not grasp either the process or the goal.

Matters concretized in an image and a system. McCulloch had worked with Magoun, Snider and others [206-209] on the reticular formation, a core of highly interconnected neurons extending all along the inner cavity of the vertebrate nervous system from the ventricles of the brain to the central canal of the spinal cord. Every anatomist regards this system with great and proper superstition. Paul Yakovlev, for example, sees the brain as a set of laminated shells within shells in true Gnostic tradition as befits a Russian, and the reticular formation is the central shell, the primeval animal. In any case, with respect to stimulating the reticular formation, not only does the brain stem have strong inhibitory and excitatory influence on reflex activity in the cord, but it changes the brain from "sleep" to "waking" state, and the other way around. Dr. Percival Bailey jokingly called the cells around the opening of the third ventricle into the aqueduct "the center of unconsciousness," for he discovered that to touch that opening with a probe through the third ventricle instantly put an animal or man to sleep. And there exists, for example, a "ball-valve" pedunculated tumor of the third ventricle which does such touching so that the patient goes to sleep instantly when turned on his back and wakes when turned on his face.

As a result of experimental work as well as clinical observations, McCulloch described this extended structure, the reticular formation, as that region of the brain wherein "command and control" were in process, - a sort of equivalent of military HQ. I think he felt that no structure had a more transparent and primitive logic than the Pentagon - nor is that meant disparagingly. The reticular formation receives input from everywhere, gives output everywhere, seems hierarchically and multiply connected internally and somehow looks as if the elements were simple and iterated, so that complexity is had more by Byzantine arabesques of connection than the intricacy of individual forms. Very military indeed!

But this image was not yet set as the target structure in 1951, when McCulloch moved with Pat Wall and me from Illinois to Massachusetts. Jerry Wiesner had joined with Norbert Wiener (at the instigation of Walter) to invite us all into the Research Laboratory of Electronics at M.I.T. as research associates. McCulloch badly wanted to get back to New England and changed his profession yet again. He gave up his full professorship of psychiatry at Illinois. Pat Wall gave up his associate professorship at the University of Chicago. I gave up nothing, because I had spent almost four years at Manteno State Hospital and was becoming almost as institutionalized as the patients.

Between 1947 and 1951, Pitts, McCulloch and Wiener were active in the Macy Foundation Conferences* - that particular section devoted to understanding the brain as a system. John von Neumann came to those sessions and I recall how Pitts and McCulloch looked forward to them with great excitement and gravity. It was a highly elite club; Ross Ashby, Heinz von Foerster, Donald MacKay, and about 18-20 others spent a week each year at a Princeton, N.J. inn exploring strategies, meanings, practices and the like. Oliver Selfridge and I once crashed a meeting out of curiosity, with a mechanism we had trumped up as an excuse to come - something about sex hormones - but we were politely booted out. These conferences had the salutary effect of creating a network for the passage of ideas about an as yet non-existent science ("experimental epistemology" as Oliver Selfridge and I called it to Walter Pitt's grimace), and also became channels of opportunity for bright students who meant to take a chance on it. When Jerry Wiesner invited us to RLE, he had in mind a center to create the science which was at the time always in the offing, like a forgotten word on the tip of your tongue. Wiener was an essential part of the dream we all shared. The reason that McCulloch gave up his chair at Illinois was to learn his way into complex formal methods under the guidance of Wiener. Wiener was to serve as a critical colleague against whom to sharpen ideas. However, in 1952 starting from a personal misunderstanding that was manipulated by the Wiener family, for reasons not germane to this essay, into a violent rift, Wiener severed all relations with our group. This dismal and unnecessary divorce colored the rest of our lives.

From 1952 on McCulloch lost interest in continuous processes and became more and more involved with the idea of - how shall I put it? - contingent logic, not so much in devising it from first principles, but adducing it from a kind of evolution of elements

* (77) von Foerster, H. *Circular Causality*.

under the pressure of system requirements. This is not the way in which most mathematicians think (Wiener was an exception in this respect), but it certainly is the way by which most engineers work. And McCulloch was a superb engineer. At the same time he almost completely lost interest in the laboratory. He constantly abjured us to keep experimenting, but he had no real concern with the actual performance, and we did not nag him to attend.

A remarkable reversal of interests occurred in Pitts and McCulloch over the rest of the decade and into the early 60's. For Pitts continued to attend the laboratory, planned experimental work, while McCulloch moved to pencil and paper. Pitts designed the experiment of calculating sources and sinks of current from an array of electrode positions inside a segment of spinal cord. Pat Wall and I did the actual cat work, and Brad Howland invented clever methods for doing all the calculation by analog computer. However, in the end, the analog machine would have taken too long to build and troubleshoot, so almost a whole year was spent in computing by hand. This was the 1955 paper [127] (Howland et al), appearing just as Whirlwind (the high-speed digital computer) was becoming accessible.

Pitts next turned his attention to smell - then a truly unstudied sense. He had the good fortune to entrain Bob Gesteland, the most indefatigable and loyal of student-colleagues. But Pitts lost interest in about two or three years, and Bob stayed with us eight more years and published the results of their work. It was not so much that Pitts was looking elsewhere; he had quite simply run down steadily since the rift with Wiener in 1952, and he finally lost interest in everything. He read incessantly and omnivorously, but stayed away from everyone. He read like someone waiting to die but willing to be distracted during the last hours. Those last hours lasted many years. He had destroyed all his work under Wiener and, except to accommodate Giorgio de Santillana, never wrote down anything again after about '57-58.

In telling this brief history of the surround, I have left out the most important parts of this essay, which are the stance and influence of McCulloch and Pitts from 1952 on, a decade after the *Logical Calculus*. As I remarked, they had reversed roles.

McCulloch had come to the collaboration in 1943 as an experimental neurophysiologist and by 1952 had taken to working only with pencil and paper on idealized structures, and never touched physiological experiment again. Pitts had come in 1943 as a purely abstract thinker, trained principally in logic, and by 1952 was insisting on the importance of empirical knowledge and professing an utter lack of interest in the new and rapidly growing logical devices. They had, it might seem, converted each other, but that was not so.

McCulloch saw clearly in the epiphany of the *Logical Calculus* the barrenness of neurophysiology as the experimental mode for testing any theory that related brain action to mental process. For imagine a processor made of logical gates - the very sort that are so common now. Imagine disposing it in several cards cabled together by plug-in terminal strips. And now conceive trying to determine how the system works and on what problem it is working by the four methods of nervous science: the anatomy (the wiring system and the fine structure of the components), the results of stimulation (putting one card or another in such a field as generates a signal in every wire on the card), the results of ablation and poisoning (pulling out or improperly biasing a card), and the results of recording (taking the time series of transients on individual gates, on the average activity of a few gates, on the average activity of many elements and wires). The more one regards these options the more absurd they seem, and it is hard to picture anyone doing such a project seriously. Only one kind of person associated with computing machines is allowed, even obliged, to work this way, and that is the repairman who must know such time series as empirics so as to diagnose norms and defects. But no line of thought leads from repairman to theory. Thus neurophysiology is an important clinical art, but it is useless for conceiving a nervous science. McCulloch had nothing but admiration for clinical arts - they are the most interesting empirics around - but he was dedicated to knowing how the brain works in the way that the creator of any machine knows its workings.

The key to such knowledge is not to analyze observation but to create a model and then compare it with observation by mapping. But the poiesis must come first, and McCulloch would rather have failed in trying to create a brain than to have succeeded in describing an existing one more fully. It is no accident, therefore, that in his circle were Marvin Minsky, Seymour Papert and others who have devoted themselves to Artificial Intelligence.

I find McCulloch's stand appropriate and have no reservation in granting that neurophysiology is hardly the way to a theory of mind-brain relation. Certainly it is far inferior to AI both in theory and practice at the systems level. It is an amusing exercise to compare proposed mechanisms, say for vision as advanced by physiologists, with the result when the model is actually built on specification. The proposed engines are dismal failures (with the sole exception of a mechanical system suggested by Bela Julesz). Still I have stayed in physiology because I am not sure that what is to be explained has been properly described. There are some awful prejudices about perceptions, memories and the like which pose such paradoxical conditions that no engine could possibly fulfill them. Often studies of physiology lead to questions in psychology that force a redescription of process. Such a redescription is more consonant with experience than earlier versions, and also more tractable to theory. That sort of change is presently occurring in the theory of color and the theory of visual texture.

McCulloch's stand was not unreasonable - it was only premature. Because, what with the Laputan precision of analytic philosophers, the received conventions of psychophysicists, and the formal psychogonies of experts on cognition (whatever that is), it is not obvious what is to be modelled. That is what drove Pitts in the other direction from McCulloch. It seemed to Pitts fairly evident that, as in Mark Twain's comment on journalism, you have to have the facts before you can corrupt them. This is a special version of Gibb's phase rule considered as an epistemological law. Whether it was reflex mechanisms in the spinal cord, or qualities of seen objects, or the classification of smells, one thing was certain; there was always a peculiar disparity between the known physiology and the description of the functions it mediated. It seemed to Pitts that physiology had a certain critical relation to psychological description, not in terms of explanation but in terms of constraints. There was no question but that once an idea occurred clearly and distinctly one could realize it by an engine - this was the thrust of the *Logical Calculus* - only it was necessary to have a reasonable idea first.

In retrospect, these two papers by McCulloch and Pitts in the early 1940's stand out sharply and uniquely against the ground of a biological discipline that was certain to ignore or reject the ideas. Research on brain, since the turn of the century, is committed entirely to empirics. Any speculation that does not organize data is considered idle. There are theories about every aspect of nervous function except mental process - theories of nerve membrane, of synaptic action, of special chemical actions on neurons, of neurosecretory functions, indeed of any material properties or of any arrangement into mechanism. But there is no attempt to account for perception or memory or intention, for how would anyone go about establishing, for example, what data signify which percept where? Thus the current philosophy rules out from study exactly these processes whose connection to nervous action can scarcely be in doubt. Nor, as more and more observations accumulate, can one suppose that by some magic transformation of quantity to quality, or by some miraculously revealing experiment, the nature of perception will emerge. That is as great nonsense as locating and describing a percept as a particular figure of nervous excitement. For these reasons physiologists avoid any mention of mind until they become emeritus and are allowed the fringe benefit of "big thinks."

McCulloch and Pitts, however, saw quite clearly that the problem was not in locating or describing the nervous actions attending one or another mental process but, rather, in showing how any process is possible, given a nervous mechanism. That had to be established first. In a sense the notion was already implicit in the Boolean algebra and in Babbage's engine. (They did not know at the time, nor for that matter, had anyone else recognized, that Leibnitz had been over that path almost three centuries ago. That was found out later by Pitts in the early '50's).

The genius of the *Logical Calculus* was the reifying of a logical machine in one stroke by axiomatizing the known properties of inter-neuronal action. Thus, even if no animal possessed such a brain, and even if nothing of the sort had ever occurred naturally, it was a possible tissue by all the standards of anatomy and physiology, and could be arranged to perform tasks that are among those classed as mental. It is interesting that nobody, to my knowledge, asked the fatal question for physiologists. Suppose one knew the input to the nerve net, and suppose one knew the net to be made of these axiomatized neurons, and suppose one could record, as in a naturally occurring brain, from single elements anywhere - how long would it take to decide, by the data, what logical operation was being done? It is astonishing how, even in relatively elementary nets, there is a large gulf between the simplicity of the process and the intractability of extended records in terms of revealing the process. (I believe there are now machine methods for handling such a problem in simple, well-defined systems, but that is beside the point.)

The McCulloch-Pitts nerve nets were the death of analytic neurophysiology, except that no one, save McCulloch, realized it. The science still kept on, but there were now subtle, empirical questions of a new sort that could now be asked in the light of theory.

McCulloch had already committed himself to his new life-work when John von Neumann delivered his elegant lecture in 1949, "The General and Logical Theory of Automata." In it he showed what McCulloch and Pitts had known from the beginning but never expressed; that most mental processes of interest cannot be completely and unambiguously defined in a finite number of words. Thus he divorced the McCulloch-Pitts nets, which he honored, from real nets in living animals. And he remarked that real objects (i.e. their representation) may not only be the simplest descriptions of themselves, but that logic itself may "have to undergo a pseudomorphosis to neurology to a much greater extent than the reverse."

This powerful, if brief, comment was thoughtful and well-meant. It has been forgotten, I think, prematurely. And von Neumann went further to emphasize the importance of finding the details of functional connectivity in the brain. McCulloch, however, knew this, and so, in a more poignant way, did Pitts. But

McCulloch, with the optimism of any Artificial Intelligence practitioner today, felt that sooner or later all complex processes had to decompose into simple percipient logical sequences. And Pitts, whose dream of great magic foundered on its own success, struck out after new empirics.

Before I go on to the *How We Know Universals* paper, I want to compare the McCulloch-Pitts net with the earlier Turing machine, of which they had not heard at the time. In abstracting neurons and arranging their connections, Warren and Walter left out an important property of any real system - the output did not connect back to the input. The nerve-nets were through-and-through devices. Although special connections could generate memory configurations, and one could easily devise active "resting states," the nerve-nets operated somewhat like a complex slot machine - input in - solution out (after a delay, of course), and there was no looping back of the process on itself, no arrangement for recursive change of the input, although both McCulloch and Pitts talked about it after publication. The nets, for all their baroque elegance, must be reconnected for each new operation. They are overly constrained. Turing's Universal Machine, an axiomatized inchworm crawling along its tape, can compute any number that can be computed by a finite number of steps; which is to say, it can compute whatever the McCulloch-Pitts nerve nets can. The complexity of connection in the nerve-net is replaced by the complexity of the program writ on the tape. But the Turing machine can generate more program on the tape and so has no upper limit on, say, computing by recursion, as with continued fractions or series, whereas the nerve-nets, not so connected, can neither increase their own fabric nor, at least in the original paper, feed back to start a new cycle. On the other hand, the Turing machine is too abstract an engine for empirical physiologists to grasp, and the McCulloch-Pitts nerve-nets have the advantage, for students of brain, in being a brilliant and easily grasped metaphor. Although, in the technical sense, the Turing paper is more general than the McCulloch-Pitts *Logical Calculus*, they complement each other marvelously and have equal weight in that the spare, formal, step-by-step nature of the one to which time does not matter at all is offset by the exuberant spatial spread of the other where everything is done all at once.

It is as sad as it is unnecessary to remark that, for the reasons sketched, neither paper affected empirical nervous science. Aside from a brief flutter of interest, no further attention was paid. But with the Turing paper and the *Logical Calculus* as gospel, groups of acolytes founded a new and growing field: Artificial Intelligence. Others, more knowledgeable of that discipline than I, will discuss the impact of McCulloch and Pitts on it (60).

The *Logical Calculus* was soon followed by the *Universals*, which is quite as remarkable a work. McCulloch and Pitts knew that the geometry of the image on the retina does not define the forms seen, nor, indeed, are the specifics of sensory reports anywhere simply related to the objects of perception. It is surprising, in nerve science, to find such general agreement on this concept, yet so few have taken account of it in practice. (That is why, when Humberto Maturana and I did the frog's eye experiments, we had the hardest time persuading colleagues and sponsors that we were reporting findings, not speculations. We were fortunate in getting enough interest from the engineering fraternity so that overnight the work went from eccentricity to fashion.) The things we see are characterized by having an invariance under the transformation of accident, so that, e.g., a running dog and a resting dog may be the same dog and the running phenomenon is the same thing as that which another dog would do. *How* we make this abstraction of an enduring identity under conditions of profound change is a major problem in the theory of knowledge; certainly it is the central problem in the theory of perception. The way of thinking about these matters was invented by Galileo and called relativity. Objects and relations are represented in a mental model, and one has the ability to view them from any direction in the model, whereas this is not possible in the real world. So I can imagine the model in many different ways by carrying a coordinate system to it along any path and choosing the coordinate system at will. But, elegant and useful as this notion is to physics, how is it possible to realize it in a mechanism?

Here again McCulloch and Pitts presented an axiomatized real system, and they showed for the first time, how the process is possible. They deal explicitly only with simple operational dilatation and constriction of a visual representation, and the translation of a chord ratio up and down the scale in audition. But they demonstrate in a clear and unambiguous way how an engine can carry an organizing frame, an imbedding space, to representations so as to order them. While this method is now the stock-in-trade of every computational facility, so that the miracle is now commonplace, the reification then was novel. Again, expectedly, little of the technical, much less the philosophical thrust of this demonstration entered into the field of physiology or psychology. For some peculiar reason, stemming from a misconception of the nature of science, neurobiologists prefer endless lists of measurements to the simpler search for invariances, since the former are reliable signs of tangible *mechanism*, while the latter smell of the more "spiritual" and suspect *process*.

One might have expected the *Universals* to have generated at least an experimental program in psychology to discover the kinds of coordinate systems that are brought to the sensory plenum. But, with few exceptions, little has been done. The weight of prejudice against speculation prevents the emergence of theory. I except only linguistics from this disappointment. On the other hand, the computational fields, notably Artificial Intelligence, have adopted and adapted the classical ideas embodied in McCulloch's and Pitts' demonstrations, and taken them to a fare-thee-well. I think they both would have been delighted in 1944 by the NASA image-enhancing programs that are now standard, surely one of the simplest and most vivid tributes to their notions. More complex applications of their approach exist now, some in use, some in the planning stage.

It would have been proper to insert here Pitts' discovery of Leibnitz as the father of the theory of automata. Unfortunately, that view of Leibnitz is almost a whole book in itself. I am engaged to write it but as yet can see no way of compressing the story without weakening it. It is not the explicit content that matters, for that can be readily told. Rather, it is the web of relations between Leibnitz' ideas and the other ideas at the time that is ensnaring - for the metaphysical tensions underlying the consideration of automata are not new.

It would also have been proper to give in more detail McCulloch's treatment of "probabilistic" logic, "majority" logic, three-valued logic, and a variety of other mutations by which he sought, through a transformation of axiomatized elements, to achieve a model of nervous action more consonant with the animate processing of a real world as von Neumann wanted. I do not think he ever succeeded, even in his own eyes, in getting a glimmer of a new principle. Yet the quest, however quixotic, was noble. In any case, I have neither the competence nor inclination to examine the various modes he treated.

McCulloch and Pitts were giants who I loved dearly and miss much. The rest of this book details Warren's accomplishments. I thought to put in a kind of revery about how I remember them both.