FREUD, S. (1940a [1838]). An outline of psycho-analysis. *Standard Edition, 23,* 144–207.

FREUD, S. (1940b [1838]). Some elementary lessons in psycho-analysis. *Standard Edition, 23,* 282–286.

FREUD, S. (1954). *The Origins of Psychoanalysis.* New York: Basic Books.

PRIBRAM, K. H. & GILL, M. M. (1976). *Freud's "Project" re-assessed: Preface to contemporary cognitive theory and neuropsychology.* New York: Basic Books.

SACKS, O. (1984). *A leg to stand on.* London: Duckworth.

SOLMS, M. & SALING, M. (1986). On psychoanalysis and neuroscience: Freud's attitude to the localizationist tradition. *International Journal of Psycho-Analysis, 67,* 397–416.

SOLMS, M. (1997). What is consciousness? *Journal of the American Psychoanalytic Association, 45,* 681–703.

T-209. PRIBRAM, K.H. (1998) A Century of Progress? In Robert M. Bilder and F. Frank LeFever (Eds.) *Neuroscience of the Mind: On the Centennial of Freud's Project for a Scientific Psychology.* New York: Annals of the New York Academy of Sciences, Vol. 843, pp. 11-19.

A Century of Progress?

KARL H. PRIBRAM[a]

Professor Emeritus, Stanford University, and James P. and Anna King University Professor and Eminent Scholar, Radford University, Commonwealth of Virginia, USA

For this symposium, I debated about what to present. Dr. Solms has pointed out that the time is ripe to realize what Freud was after: to build models based on neuroscience regarding conscious and unconscious processes. So I could have presented a current model developed in my recent book *Brain Perception: Holonomy and Structure in Figural Processing* (1991). Or, I could have shown some current data from the lab at Radford, where my colleagues Joseph King, Min Xie, and Bibo Zheng, and I have been able to do some fascinating work with microelectrodes in rats and with EEGs in humans. I've developed a new technique for analyzing running EEGs, and Don Tucker has just installed a 128-electrode array. I could as well have dealt with new developments in brain theory, developments based on quantum holography, which I call *holonomy*. However, we are here to talk about Freud's *Project*, and so I am going to take on Dr. Solms and disagree with him on certain issues. Mark Solms insists that Freud's essay "On Aphasia" is more indicative of his neurological thinking than is the *Project*, and I do agree that "On Aphasia" is seminal. However, the *Project* is infinitely more detailed and encompassing regarding neuropsychological topics other than language and is indeed the Rosetta stone for psychoanalytic theory and for psychoanalysis as a language-based practice.

My first paper on the *Project*, "The Neuropsychology of Sigmund Freud," (1962), showed how the *Project* provided a Rosetta stone for the terminology used in psychoanalysis. For instance, until I'd read the *Project*, I wondered "What is 'cathexis'?" Freud never used the word, and there seemed to be so much other verbal garbage in psychoanalysis that I always felt that it had nothing to do with anything scientific. But then, because of Jones' biography of Freud, and with some prodding from Jerome Bruner, I read the *Project* and suddenly felt that I knew what psychoanalysts were talking about.

I take exception to the idea that Freud gave up the *Project*. Of course, he repeatedly said he was dissatisfied with it, just as I, right now, often feel like giving up on thinking about the neural concomitants of consciousness. But as noted in *Freud's Project Re-Assessed* (Pribram & Gill, 1976), Freud repeatedly rekindled his interest in neurology. Furthermore, once a theory is formulated it stays with one. I claim this especially for Freud's theory. Look at the introduction to *Freud's Project Re-Assessed:*

> One evening last week when I was hard at work, tormented with just that amount of pain that seems to be the best state to make my brain function, the barriers were suddenly lifted, the veil was drawn aside, and I had a clear vision from the details of the neuroses to the conditions that make consciousness possible Everything seemed to connect up, the whole worked well together, and one had the impression that the thing was now really a machine and would soon go by itself. The three systems of neurones, the free and bound state of Quantity, the primary and secondary processes, the main tendency and the compromise tendency of the nervous

[a] Address for correspondence: Brain Center, Radford University, Box 6977, Radford, Virginia 24142. Phone, 540/831-6108; fax, 540/831-6236; e-mail, kpribram@runet.edu

> system, the two biological laws of attention and defense, the indications of Quality, Reality, and Thought, the (particular) position of the psychosexual group, the sexual determinant of repression, and finally the necessary conditions for consciousness as a function of perception: all that was perfectly clear, and still is. Naturally, I don't know how to contain myself for pleasure. (Freud, 1895, letter to Wilhelm Fliess, 20 October)

If you have ever read Sullivan on paranoia, you will read an almost identical description of a paranoid process coming on. A theory is like a paranoid process. Once it gets hold of one, it's awfully hard to get rid of it. Freud, being more paranoid-schizo than paranoid, almost immediately saw fallacies, loopholes in his arguments, and incompleteness in this *Project*. Therefore, although he was dissatisfied, he certainly did not give it up—witness chapter 7 of the *Interpretation of Dreams* (1900).

I don't think Freud failed to publish the *Project* for the reasons that everybody thinks he did. I have my own ideas on this topic, and they are pure conjecture: Freud was up for tenure, for a faculty position at the University of Vienna, and so was Exner. In 1894 Exner published a "Project for Scientific Psychology." Freud and Exner were both writing at the same time. Exner beat Freud to publication and also got the position. Now Freud had the option of saying "Hey, my Project is better than your Project," or just letting it go. It was an awkward situation. I believe that Freud thought it unthinkable to publish his own version of a project after having not gotten the academic position. Furthermore, as Dr. Solms noted, Freud was thrown back on his clinical practice and had to limit his research to his patients' verbal reports of introspection. He had no laboratory, and neurology was gone from his life. These are the factors, I believe, that made it necessary to abandon the *Project*.

Medical people then, just as today, felt more at home with brain "facts" than with psychology. Freud, as a scientist, therefore needed to make the case that psychology could be a science in its own right, and not just a neurological discipline. We are faced with the same problem today. Abandoning neurology posed a philosophical problem for Freud: Today's establishment, in the role of eliminative materialists such Francis Crick, the Churchlands (Paul and Patricia), and Steven Stitch, want to get rid of what they call "folk psychology." They claim that if you know all about neurons you don't need to have a science of consciousness; once we know what each neuron does, we have it all. They are, of course, making a "category error," but Crick passes that objection off as just philosophical nonsense. No turn of the century Viennese was that *unsophisticated*. Nor do we need to be now. Last year, we at Radford's Center for Brain Research and Informational Sciences hosted a conference entitled, "Scale in Conscious Experience: The Brain is Too Important to Be Left to Specialists to Study." (King & Pribram, 1995). We took an anti-elimination stance because that stance is just too awful to let go by without challenge.

NEUROSCIENCE IN 1895

Mark Solms has alluded to what neuroscience was like at the end of the nineteenth century, saying that so little was known. I totally disagree with his assessment. When, in the *Project*, Freud developed the concept that the key neurons at the base of the brain secreted an adrenaline-like substance into the blood stream, I wondered how he could know something like that. Then I remembered that my father had done his thesis at the Charles University in Prague on the relationship between the hypothalamus and the pituitary gland and about how the secretory neurons regulated the pituitary. This was in the 1890s. Vienna at that time was full of practitioners who talked about sympatheticotonia and parasympatheticotonia. What I like about the *Project* is that it has so much detail in it. At the time he wrote the *Project*, Freud emphasized not the drive basis of motivation, but its basis in the memory-motive structure developed in the core brain (the basal forebrain), *not* the cortex. Only later, when Freud began to believe that memory became distorted by the surge of hormones at puberty, did he ascribe an overwhelming importance to drives (by that time called the id—the "it" of Clara Bow in the silent movies).

Clark Hull took up Freud's later emphasis on drive and applied it to learning. I was at Yale at the time that drive-reduction theory was rampant in psychology. Drive was considered an intervening variable in stimulus–response formulations, but Neal Miller found that hypothalamic lesioned rats could gorge themselves when fed *ad lib*, but would starve if they had to work for food. These rats had an "increased drive" in one condition and a "decreased drive" in another. Bill Estes (in Sigmund Koch's *Psychology: The Study of a Science*, to which I also contributed) wrote that the intervening variable notion of drives had become untenable, but that we could think of drives as stimuli coming from inside the body. Estes' definition is consonant with Freud's in the *Project*, where he described *Triebefedern*, driving forces as chemical inputs to the central nervous system. Hullian's stimulus–response theory foundered on the drive-reduction issue and sank when it became obvious that curiosity and effort were critical motivators. I was never enamored of stimulus–response formulations and was therefore delighted to find in the *Project* a memory-based theory of motivation that was consonant with the views Miller, Galanter, and I (1960) put forward in *Plans and the Structure of Behavior*.

Furthermore, with respect to the so-called ignorance of neurology, everybody in Vienna "knew" that the cortex was the seat of consciousness. I myself thought this to be wrong, until Lawrence Weiskrantz (one of my more successful students and head of the department of Experimental Psychology at Oxford) discovered "blind sight." A patient with an occipital lobe hemangioma underwent neurosurgical removal of the tumor with the resultant contralateral hemianopia. Weiskrantz tested him with different stimuli, such as circles and squares, instructing the patient to point to them to guess what they were. Weiskrantz had been interested in characterizing residual vision having tested a long series of monkeys. The patient performed at about 95 percent correct in his pointing, and about 80 percent on guessing the shape of the display pointed to, and so Weiskrantz exclaimed "You're lucky, your vision is coming back." But the patient said: "I don't see anything." So then Weiskrantz repeated the examination, presenting squares, triangles, and circles—all fairly large—in the blind visual field. The patient performed at the 80–85 percent correct level. Still the patient claimed that he didn't see anything in that field. This is blind sight. There are other syndromes where verbal reports of introspection and their nonverbal behavior do not match up. These findings made George Miller, Gene Galanter and me (1960) exclaim with laughter, in the epilogue to our book *Plans and the Structure of Behavior*, that we were "subjective behaviorists," an oxymoron that proclaims that one cannot eliminate conscious subjectivity if one is going to do neuropsychology.

To go back to this idea that so little was known at the end of the nineteenth century: If you compare how people thought about the brain and psychology to what they thought at the end of the eighteenth century, the strides made from 1790 to 1890 were so great that they put our century to shame. When you consider William James' book, *Principles of Psychology* (1950) along with Exner's and Freud's projects, all the fundamental ideas are there. In the epilogue to *Brain and Perception*, I suggest:

> Looking back at the development of the holonomic theory of brain function as presented in these McEachron lectures, I am constantly surprised by the following paradox: Giant strides appear to have been made in understanding figural

> perception during my research career. At the same time, the fundamental approaches that are of a sufficient magnitude to now constitute a paradigm shift were already accepted a century ago by Poincaré (1905), Helmholtz, and Lie. Of course, real strides have been taken during the past 50 years: A multitude of experimental results have supported the earlier views and these views have been refined with the more sophisticated computational tools now available. But perhaps the apparent magnitude of the strides is due to the fact that there has been an intermediate period wherein another, rather different, view of the perceptual process held sway: a view in which higher order perceptual complexities of form are synthesized from points and lines. The interim view contrasts sharply with the earlier one and to the lectures presented here. (pp. 269–270)

If Helmholtz and Ohm (of Ohm's Law of electricity) were around today, they might ask "So what have you learned that's new?" There's a letter from Helmholtz to Poincaré, asking "How do we perceive objects, [and] what kind of a mathematical treatment can I give it?" and Poincaré writes back to Helmholtz saying "use group theory." Helmholtz does it, and publishes, and then Lie, a Norwegian, writes to Poincaré and says: "What did you tell this Helmholtz about group theory?; he used the wrong group theory . . . it won't work . . . He used discontinuous groups, you've got to have continuous groups to form the perception of objects. I invented continuous group theory just to solve this problem." This is 1886. I would ask Dr. Solms whether he really thinks that so little was known. Freud admired Helmholtz to such an extent that he wanted to model the *Project* along just such "physicalist" lines—and in doing so, he developed an Ohm's Law of neuronal processing which, on occasion, has been misinterpreted as "hydraulic" (because electricity was talked about in those days as "flowing from a source of greater to one of lesser potential") (FIG. 1).

THE *PROJECT* AS NEUROSCIENCE

Another thing Dr. Solms' mentions concerns Freud's method, and this brings me to a story: One evening at a dinner hosted by the San Francisco Psychoanalytic Society, I was seated between Ken Colby and Alan Newell. I asked them to compare the psychotherapeutic process to that by which computer scientists program chess games. I

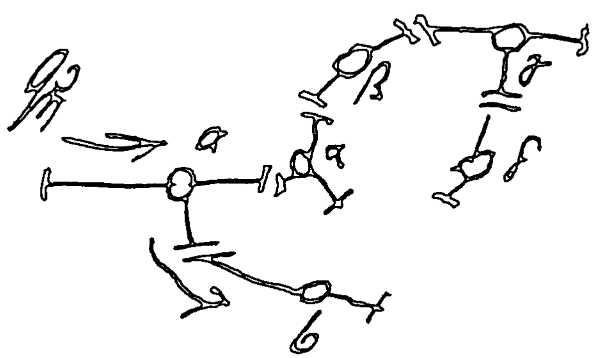

FIGURE 1. Freud's diagram of the manner in which connections are established in dreams.

asked: Doesn't the computer scientist develop a "theory," test it against an opponent, note the theory's failings, incorporate a solution provided by the opponent, test the amended theory against a more proficient opponent, and then repeat the process until satisfactory winning strategies and tactics are developed? Substitute *psychotherapist* for *computer scientist* and *patient* for *opponent* and doesn't this describe the therapeutic process? Newell agreed to the correctness of the chess analogy; Colby stated that was exactly what he was doing (which I knew, but Newell didn't) in simulating, by computer program, his therapeutic procedure and testing it against his patients' productions the following week. We all agreed as to the similarity of the two processes: *ergo*, either psychoanalysis as Freud proposed it in the *Project* and psychotherapy are indeed both "scientific" procedures or else computer programming as used in developing chess strategies *fails* to be "scientific."

Next, I want to address the issue as to whether Freud's metapsychology is indeed a neuropsychology. Freud defined his clinical theory on the basis of his interactions with his patients. The metapsychology, he argued, went beyond the clinic into neurology and cultural concerns. In his later years, Freud became especially interested in the sociocultural aspects of psychology, but his earlier metapsychology was truly a neuropsychology.

When we were trying to decide what to call this field of enquiry, I was working in Lashley's laboratory, as was Don Hebb. The question was, should we call it biopsychology, psychoneurology, or neuropsychology? I liked neuropsychology, because it was what I was after. I wanted to know how the brain works, and one of the ways of finding this out is to perform behavioral tests, including those using verbal reports of introspections. Neuropsychology, therefore, is the same sort of interdisciplinary endeavor that biochemistry is, where one uses chemical techniques to find out about biology. So in neuropsychology one uses behavioral techniques to find out how the brain works. That's why I liked the term, and Hebb liked it for the same reason, though he avowed that the CNS he got to know was only a "conceptual nervous system."

At that time, we didn't know what name would stick and, in fact, there are other names that are used. *Psychobiology*, for instance, which is the use of biological techniques to learn about the psyche. One never knows what term will turn out to be accepted: in my school days we used to talk about physiological chemistry, a term that has pretty well dropped out. Everything is biochemistry now. According to this analysis, Freud really did develop a neuropsychology at the time of the *Project*, because at that time he was interested in using his psychological insights to understand brain function. Only later did he drop the *neuro* to construct a "psychology in its own right."

Thus, a major reason for my deep interest in the *Project* is that Freud presented a sophisticated neurophysiology—much more so than he has been given credit for. Freud emphasized graded local field potentials—it was these that were translated as *cathexis*. I wondered how nineteenth century neurology was so sophisticated, more so than the neurology of the first half of this century. Finally, I realized that, at the time, all neuroscientists knew that there were two kinds of processes discernable in neural tissue. Freud reflected this knowledge in the *Project*. One process depended on nerve impulses. These were propagated and called action currents, translated by Freud's translators as "currents in flow." The other process, measured as drifts by galvanometers, were local potential changes, changes in voltages, translated as cathexis. Then, finally, there were resistances at contact barriers, what today we call synapses. The resistances were worn down by use when both the pre- and postsynaptic site were activated. Today this is known as Hebb's rule—because a half century later Don Hebb also came to the conclusion—spelled out in considerable detail by Freud: that *selective* learning depended on pre- as well as postsynaptic change as a result of activation.

Note, as I indicated earlier, that by taking local field potentials—cathexes—and synaptic resistance into account. Freud had formulated a qualitative form of Ohm's Law. To discover Freud's formulation of an Ohm's Law in the *Project* was exciting for me. I had, since the 1950s, been trying to get the scientific community to realize that in addition to circuits dependent on propagated nerve impulse transmission, local field potentials were an important ingredient in brain functioning. Now I asked myself: "Why would the scientific community neglect local field potentials?" The answer is simple: In the 1890s scientists didn't have push/pull amplifiers, but made their measurements with galvanometers, and so they didn't get rid of drifts in their recordings. They were measuring these local graded potentials, considered in later times as artifacts that get in the way. But these graded local field potentials are not artifacts.

If you look around, everything you see is processed in the retina by graded local field potential changes. There are no nerve impulses in the retina until you get back to the ganglion cell layer. Hodgkin, who received the Nobel Prize for devising a model of nerve impulse generation and conduction, gave the keynote address at an international physiological conference in New Delhi. He said: "After I received the Nobel Prize, I began to wonder—is this what I went into neuroscience for? Certainly not. I wanted to know how the brain works." So he puzzled for a while, then had a brilliant idea: "Ha . . . I don't need to get through the skull to study the brain—there is a piece of it made readily available just for neurophysiologists to study. This piece of brain is called the retina." So Hodgkin put his probes into the retina with the result that: "For ten years now, I've been studying retinal processes, and I haven't seen a nerve impulse yet." Local field effects *are* important—they allow brain computational processes to take place that nerve impulse circuitry does not.

Sir Charles Sherrington noted that the more reflex a behavior, the more automatic, the less "mind" accompanies it. There seems to be an antagonism between mind and automatic behavior that is mediated by nerve impulse circuitry. If my brain processes depended on nerve impulse circuitry, I could not stand here and think "to be or not be." "To be or not to be," that is *not* the question: the question is: What is going on inside my brain that allows me to think? There must be a delay between a sensory input pattern that reaches the kind of network that is in the retina and the cortex and an output into neuronal circuits that regulate behavior. Retinal delay is 60 milliseconds: we are considering substantial delays. These delays are what Freud was talking about, his cathexes. So when Freud describes a shift in cathexes, he is talking about the same sort of process going on in the brain that goes on in the retina.

Dr. Solms has claimed that in 1895 everybody knew about neurons, but that is not so: the neuron doctrine was a very controversial and hot topic at that time. Cajal said *yes*, the brain is made up of independent elements, neurons; Golgi said the cortex was a syncytium, a network. The issue could not be settled on an anatomical basis, and, even with electron microscopy, I don't believe that the issue could be settled today. It took Sherrington's insight that neuronal function *per se* could not account for reflex behavior—that directional synapses must be involved—to settle the issue in favor of neuron theory. By the way, even as late as 1940, I was asked to discuss the "neuron doctrine" in my final exam in neuroanatomy. Once more, I want to emphasize that Freud presented his views in great detail, accepted the controversial neuron doctrine, and showed its consequences for learning and perception. I maintain that, given this amount of detail, he could not have gotten rid of his insights, though he abandoned his intention of doing any further work on them at the neurophysiological level.

Even at a neural systems level, Freud produced an amazing amount of detail: he described a *double* feedback between basal forebrain and cortex to produce the attentional process necessary for reality testing; the memory-motive structure (with neuronal diagram) in the basal forebrain that underlies a wish; the equality of influence of drive, self-help, and caretaker (superego) in the development of ego in basal forebrain. FIG-

URE 2 is the frontispiece of *Freud's Project Re-Assessed* and outlines ω (cortex), ψ (basal forebrain), and φ (sensory-motor systems).

FREUD AS A SCIENTIST

For the reasons outlined, Freud dropped his study of neurology and went on to try to formulate a psychological science in its own right. Skinner did the same thing a half century later. As did Freud, Skinner received his doctorate in biology. But he also felt that we have to create a science of behavior on its own ground because by neurologizing prematurely, we are going to be misled. As I have noted elsewhere (1965) I don't wholly agree with either Freud or Skinner on this. Freud's neuropsychology, his neurologizing, was pretty darn good. And Skinner proclaimed in 1989, a year before his death,[12] that:

> There are two unavoidable gaps in any behavioral account: one between the stimulating action of the environment and the response of the organism and one between consequences and the resulting change in behavior. Only brain science can fill those gaps. In doing so it completes the account; it does not give a different account of the same thing. (p. 18)

Why then, when Freud was doing what later Behaviorists also espoused, has Freud become so controversial? "Freud is right! Freud is wrong!" Why don't scientists say this about Pavlov? Pavlov is revered as a neuroscientist even though he never had one experiment on brain function succeed: In his 1924 book, he lamented that all his dogs got brain abscesses or epilepsy, that he never had one decent result. And nobody says "Pavlov? Ugh! Pavlov is wrong! Pavlov is right!" What about Don Hebb, the current

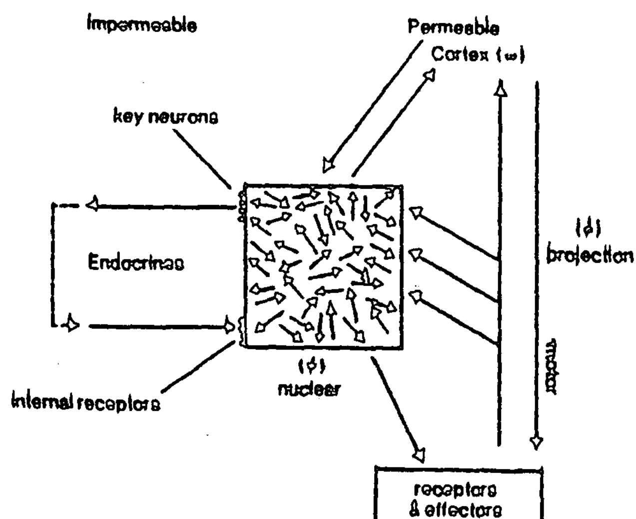

FIGURE 2. Stylized representation of the "machine" or "model" of psychological processes presented in the Project.

hero? As I indicated, Hebb stated that for him the CNS meant "conceptual nervous system." That's what Skinner disliked about it—that it was all conceptual neurologizing. Why is it that Freud is not esteemed as the neuroscientist that he was?

In the early 1960s, while I was in transition from Yale to Stanford, I didn't have my lab functioning as yet, and I lectured on the *Project* as if it were my own model of neural processing. My lectures were well received; I got good questions and discussions, and in fact, a good deal of excitement was generated. Both faculty and students in psychology and neurophysiology felt that I was onto something. After the discussion period at the end of each session, just as we were about to disband, I stated: "Oh, by the way, this is not my model—it's Sigmund Freud's, which he devised in 1895." No one would believe me, nor did many psychoanalysts years later after Merton Gill and I had published *Freud's "Project" Re-assessed.* Only the Washington-Baltimore and the William Alison White schools accepted our insights. As for most others, it was always that I was reading into Freud what I knew on the basis of what we now know in the neurosciences. Merton Gill had the same problem as we wrote our manuscript: I had to prove to him at every step by showing him quotations, that indeed it was Freud—not Pribram—saying these things.

Why this reluctance to believe? Why is Freud considered so differently than are Pavlov or Hebb? I believe the answer is simple. Pavlov and Hebb couched their neuropsychological speculations in neuroscientific terminology—*voilà*, they are neuroscientists. Freud, by contrast, couched his neuropsychology in psychological, subjective terms. For instance, in the *Project*, he repeatedly reminds his readers that the "generation of unpleasure" is due to a biochemical process during which the key neurons secrete an adrenaline-like substance, which, in the blood stream, produces excitation leading to further secretion of adrenaline by the adrenal gland. In turn, this excitation results in a drive-stimulus to which the nervous system is sensitive, leading to further excitation and discharge through the key neurons. Defenses are constructed to prevent this vicious biochemical process from getting started.

Later, this neurobiological process became the basis for the unpleasure principle and finally for the pleasure principle! Its neuroscientific origin is now long buried and therefore becomes untestable in its current guise. My interest in the *Project* is in baring the roots of this principle and the others that are the currency of psychoanalysis (such as cathexis) in order to be able to welcome psychoanalysis (and psychotherapy in general) back into the natural sciences (Pribram & Gill, 1976).

Of course, there is a great deal of artistry involved in both therapy and in computer science as there is in all science. But my question is: When are we hard-nosed neuroscientists going to stop looking down our noses at the "softies" in the clinical and social fields? There is no study in experimental psychology or sociology that could get by without indicating the statistical reliability of the data. But no one in the hardest of hard neurosciences, say, extracellular recording, even discusses the sampling problem (we can record only from the largest of neurons, perhaps 10 percent of the population in the cortex). In fact, I was the first ever to publish a statistically reliable result in the sensory evoked brain electrical potential literature; I used a median (to get rid of outliers) of 20 consecutive recordings! And when I began to use computers in the 1960s to reliably plot (two standard deviations above background activity) receptive field configurations in thalamus and cortex, I was castigated during a presentation at MIT for bringing ruination to neuroscience placing a computer between the experimenter and the neuron. I am sympathetic to such concerns, but want to point out that all of us are in the same boat, trying to learn what our world is all about in as "hard-nosed" and reliable a way as possible without becoming so limited by our techniques that we find nothing of value.

By relaxing our view a bit, we find that Freud, 100 years ago, was in fact a good scientist and what his science had to offer—even in considerable detail—is not so different from what we today can offer. There is truly a tradition in neuropsychology, a tradition shared by Freud, which, over the past *two* centuries has developed a fairly valid but testable and modifiable picture of the mind/brain relationship. Much of this picture was developed in the nineteenth century and has been passed on to us in Freud's 1895 *Project for a Scientific Psychology*.

REFERENCES

EXNER, S. (1894). *Entwurf zu einer physiologischen Erklärung der psychischen Erscheinungen.* Vienna: Franz Deuticke.

FREUD, S. (1895/1966). *Project for a scientific psychology. Standard Edition.* Vol. 1, pp. 281–397.

FREUD, S. (1900/1953). *The interpretation of dreams. Standard Edition.* Vols. 4–5.

JAMES, W. (1950). *Principles of psychology,* Vol. 1 and 2. New York: Dover Publications, Inc.

KING, J. S. & PRIBRAM, K.H. (1995). *Scale in conscious experience: Is the brain too important to be left to specialists to study?* New Jersey: Lawrence Erlbaum Associates, Inc.

MILLER, G. A., GALANTER, E. & PRIBRAM, K.H. (1960). *Plans and the structure of behavior.* New York: Henry Holt. [Russian translation; also in Japanese, German, Spanish, Italian].

POINCARÉ, H. (1905). *The value of science.* [Reprinted 1958. G. Halsted, trans.]. New York: Dover Publications, Inc.

PRIBRAM, K. H. (1962). The neuropsychology of Sigmund Freud. In A. J. Bachrach (Ed.). *Experimental foundation of clinical psychology.* New York: Basic Books, pp. 442–468.

PRIBRAM, K.H. (1965). Freud's *Project:* An open biologically based model for psychoanalysis. In N.S. Greenfield & W.C. Lewis (Eds.). *Psychoanalysis and current biological thought.* Madison: University of Wisconsin Press, pp. 81–92.

PRIBRAM, K. H. (1991). *Brain and perception: Holonomy and structure in figural processing.* New Jersey: Lawrence Erlbaum Associates, Inc.

PRIBRAM, K. H. & GILL, M.M. (1976). *Freud's "Project" re-assessed: Preface to contemporary cognitive theory and neuropsychology.* New York: Basic Books.

SKINNER, B.F. (1989). The origins of cognitive thought. *American Psychologist, 44*(1), 13–18.