

FREUD'S SCIENTIFIC BEGINNINGS.

by

Siegfried Bernfeld, Ph. D., San Francisco.

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The childhood phantasies and the adolescent day dreams of Freud, as far as we know them, do not foretell the future originator of psychoanalysis. They fit a general, a reformer or a business executive rather than the patient, fulltime listener to petty complaints, humdrum stories and the recounting of irrational sufferings. It was a long way from the child who devoured Thier's story of Napoleon's power; who identified himself with the Marshall Massena, Duke of Tivoli and Prince of Essling, to the psychoanalyst who cheerfully admits that he has, in fact, very little control even over those symptoms and disturbances which he has learned to understand so well. Twelve years old, he still thinks of himself as a candidate for cabinet rank and, as an adolescent, he plans to become a lawyer, and to go into politics. Then, at seventeen, shortly after his graduation from high school, Freud suddenly retreats from his search for power over men. "The urge to understand something about the mysteries of the world and maybe contribute somewhat to their solution became overwhelming". (1) He turns to the more sublime power over nature, through science, and he decides to study "natural history"—biology to us today. Power, prestige and wealth should come to him only contingent to his being a great scientist.

Great he had to be. "I fear mediocrity," he says in a remarkable letter to a friend, in the days of the final examination. (2)) This friend had recently tried to console him: "He who only fears mediocrity is quite secure". "But," answers Freud, "at night—June 16, 1873 . . . with a somewhat somnolent philosophy. . . . Secure from what, I

must ask; certainly not secure and assured that one is not mediocre? What does it matter whether you fear something or not? Isn't it most important that what we fear is true? Quite true that even stronger minds are gripped by doubts of themselves; is therefore anyone who doubts his own merits a strong mind? He may be a weakling in intellect, but an honest man withal—because of education, habit, or even self-torture. I don't want to ask you to mercilessly dissect your reactions whenever you find yourself in some doubtful situation, but if you do it, you will see how little certainty there is within you. The magnificence of the world is founded on this multitude of possibilities, only that is unfortunately no strong basis for knowing ourselves."

In the fall of 1873, with high ambitions and vague ideas and plans, he registered at the University of Vienna. He chose the medical department (Medizinische Fakultät) which combined what we call here the pre-medical curriculum and the medical school proper. It was the place of training for physicians as well as for future research men in biology—the field in which Freud's hopes lay. In sharp contrast to the closely supervised and rigidly regimented life and learning at the gymnasium (high-school), the university offered an almost complete freedom from disciplinary rules. Students, who like Freud, craved for knowledge could satiate their thirst freely, without concern for grades and credits, in any one of the many lectures, seminars and labs. Few rigid requirements were laid down, and between the Matura (graduation from high-school) and the first comprehensive examination for the M.D. the student could enjoy several years of unmitigated "freedom of thought", a condition of which Freud took good advantage, in his first three years. He indulged in varied and chaotic studies and, as he has repeatedly confessed, turned out to be a dismal failure, particularly in chemistry and zoology. In his third year he settled down in Brücke's Institute of Physiology and, with few interruptions, staid there for six years. He passed, in 1881, with great delay, but in one stretch, the examinations required for

the M. D. diploma. In 1882, when he was twenty-seven, he left Brücke's Institute for economic reasons and prepared himself for private practice in neurology. During these nine university years Freud published five scientific papers and the translation—from English—of a volume of essays.

These scientific beginnings Freud has treated summarily with a few lines in his autobiographical writings. Brun (3) and Gray (4) have carefully listed them and Brun, (3) Dorer (5) and Jeliffe (6) have given a brief evaluation of some of them. In the following pages I will give a more thorough report on them together with information I have been able to gather on the scientific situations in which they were planned and written and on Freud's teachers and their institutes. (7) I have tried to evaluate the merits of the papers in their time and their place in Freud's scientific evolution. I will not limit myself strictly to the 1873 to 1882 period but will include in this study four papers which were written and published during the years 1883 and 1884, for they belong to Freud's beginnings as elaborations or continuations. I will further discuss his physiological efforts in 1878 and 1883 which have not resulted in papers and are unknown to Brun, (3) Jeliffe, (6) Gray (4) and Dorer. (5)

I. ZOOLOGY.

Freud's efforts in zoology resulted in a paper on the testes of the eel. (8) This first scientific study by Freud, though his second publication, provides the opportunity to confront Freud's deprecatory judgment about himself as a zoologist with an opinion independently arrived at.

The sex life of the common eel had been a puzzling problem since the days of Aristotle; in 1876 it still seemed unsolved. "No one ever has found a mature male eel—no one has yet seen the testes of the eel, in spite of innumerable efforts through the centuries". (8) In 1874 Dr. Syrski had announced the most recent solution. He had discovered a small

lobed organ and described it as the testes of the eel. Carl Friedrich Claus, chief of the Institute for Comparative Anatomy in Vienna, assigned to his student Freud the task of checking Syrski's observations. (Though Claus' main interest was in coelentera and crustaceae, the problem of the eels was closely linked to his own earlier studies on hermaphroditism in animals.) (9) Freud dissected 400 eels, finding the Syrski organ in many of them. On microscopic examination he found the histological structure of the organ such that it could well be an immature form of the testes, though he found no definite evidence that this was the case. (8)

This study is inconclusive. Although it is written in a precise and animated style, always self-assured—at places, even cocky—its content is neither exciting nor brilliant. Still it is by no means proof as Freud asserts “that the peculiarities and limitations of my gifts denied me all success in many of the departments of science into which my youthful eagerness had plunged me. Thus I learned the truth of Mephistopheles' warning: ‘It is in vain that you range around from science to science; each man learns only what he can learn.’—*Faust*, Part I.” (10)

Claus obviously thought well of the young scientist. In the fall of 1873 Claus had come from Goettingen to Vienna with the intent and assignment to modernize the zoological department. One of his pet projects was a marine laboratory, and in 1875 he succeeded in founding the Zoological Experiment Station in Trieste, according to the patriotic official *History of Zoology and Botany in Austria*, “one of the first institutions of its kind in the world”. Claus had sufficient funds at his disposal to send a number of students to Trieste for several weeks of study and research twice a year. Among an early group, in March 1876, was Freud. Such a trip to the shores of the Adriatic, at the department's expense, was certainly much sought after and the assignment was valued as a reward or a distinction. In fact, Anna Freud Bernays remembers, more than half a century later, this grant as an important episode in the life of her brother Sigmund. (11)

In September of the same year Claus renewed Freud's assignment. He procured for Freud the needed eels of the larger size which appear only late in the season from October through January. On March 15, 1877 Claus had already presented Freud's paper to the Academy of Sciences and had it published in the April issue of the Bulletin of the Academy. Of course he would have enjoyed announcing that his institute had solved the old problem of the reproduction of eels for good. Yet he knew too well how progress in science inches ahead in a long succession of just such indecisive, unexciting little papers. Nothing shows that he was disappointed in his student's work.

In fact, Syrski's claim soon was confirmed. The lobed organ which he had discovered is the testes of the eel. Freud's paper was the first of a series which accumulated the evidence. (12) But this did not change Freud's hostile attitude toward his first scientific study. Twenty years later Freud had privately printed the list of his thirty-eight scientific writings in order to set forth his scientific merits in the hope of furthering his promotion to the position of professor *extraordinarius*. Though the abstracts were brief, never exaggerated, sometimes even understated, they did put forward the results, the new findings or new aspects of each of the items. Regarding the first paper on this list he says: "Dr. Syrski had recognized a lobed . . . organ as the long searched-for testes of the eel. On the suggestion of Professor Carl Claus I have, in the Zoological Station in Trieste, investigated the occurrence and the histological structure of this lobed organ." (13)

This is not merely a modest understatement. Were this a review by a colleague, the author would be justified in complaining of malicious falsification. In the meantime Freud had obviously learned that Syrski's discovery had been recognized by the zoologists, due to his own investigation, among others. His abstract, however, leads one to believe that Syrski's recognition occurred prior to Freud's study; and under this assumption, of course, his paper appears to be

utterly futile, aimless, and pointless, for which, in revenge, Claus bears the responsibility. (This abstract, it should be noted, is the only one in which the suggesting teacher is mentioned!)

This condemnation of his own zoological efforts, which the student felt so intensely and the old man never corrected, seems even stranger when we consider that in method, general scientific goals and spirit, the institutes of Claus and Brücke were alike. The studies in the comparative anatomy of the nervous system which Freud conducted to his own satisfaction under Brücke differed in topic only from his research in zoology.

Was the topic so repulsive to him that he felt devoid of the ability to deal with it? The eighteen-seventies were prudish and hypocritical and the moral standards of Freud's family were strictly Victorian; Freud shared them with conviction. In her old age, his sister still complains that he had not permitted her to read the improper writers, Balzac and Dumas. (11) Or is it just one of those strange coincidences that the discoverer of the castration complex wrote his very first paper on the missing testes of the eel, and let almost twenty years go by before he gave sexuality another scientific thought?

Or was, perhaps, the teacher and the atmosphere in Claus' Institute the source of his discontent? Of Brücke's Institute Freud says: "Here I found the teachers whom I could take as my models", (10) intimating clearly that the conditions for finding himself and his talents had been lacking in the preceding years. Claus was a scientist of great reputation; "his works in zoology . . . take the first place amongst the zoological text books of the present day" says Adam Sedgwick in the English translation of Claus' "Zoology". He was a very stimulating teacher—ambitious, intent and able to create emulation in his students. He was, like Brücke, a Darwinist, a conscientious worker and an ardent physicalist; narrower and of smaller scientific stature than Brücke, but not narrow and not small. Of course, the ways of ad-

miration and affection are mysterious, at least, as long as we cannot enlist the cooperation of the subject for a psychoanalytical investigation of his preferences. However, we might guess at one factor in the complex picture though we cannot estimate its relative weight. Brücke was Freud's senior by forty years, while Claus held his powerful position as a comparatively young man only twenty years older than Freud. Brücke was the contemporary of Freud's father. Claus was the same age as Freud's half-brother. These are irrelevant data which ought not to influence one's success or failure in any given field. They ought not—quite true—but they do, in the average student as well as in the singularly gifted one. From Freud's self-analysis we know that in his early childhood in Freiberg he concentrated all his love, admiration and trust on his father, and had shifted his distrust and rebellious and hostile attitudes to the brother, yet without ceasing to love him. (14) The young man accepted guidance and criticism from the old Brücke—"the greatest authority I ever met"—as he had admiringly and with awe looked up to his father in those early childhood years in Freiberg. Toward the younger Claus he may have felt that same mixture of love and hostility, of admiration and distrust, which had colored his relationship to his half-brother. Tempted to rebellion and competition, inhibited by the wish to learn and by genuine appreciation for the teacher's ideas and achievements, he lived in an irritating atmosphere full of frustration, doubts and comparisons. This was sharply contrasted by the inner peace in which he could learn and grow to self-esteem under an authority which was unchallenged and unsuspect. How to grow away to full independence from such an authority in later years then becomes a problem which has destroyed and distorted many a talent; but not Freud's. In 1876, when he exchanged zoology for physiology, this test was still six years away.

II. HISTOLOGY OF THE NERVE CELL.

The Viennese medical student of the Seventies was re-

quested to attend the classes of Ernst Brücke on “physiology and higher anatomy”, and was expected to work at least one short term in Brücke’s Institute of Physiology. Freud was little concerned about requirements, but in search of a teacher and a field for his ambitions he tried Brücke and stayed on for six years. “I was stuck there”, as he puts it. In this institute he not only worked to his own and Brücke’s complete satisfaction, but what he experienced there was of such singular importance to him that in his autobiographical comments this period of his life is the one of which he speaks in unrestrained superlatives as “the happiest years”. What the reasons for this gratitude were we do not know. But we can say with certainty, that it was during these six years that Freud acquired or developed to maturity those qualities which were to become characteristic of him as a scientist.

It has been shown that in fundamentals as well as in many details the Freudian concepts and theories have their roots in the Brücke Institute; that, to a certain degree, they are transformations of the ideas and methods Freud had learned there. (15) This justifies my giving in extenso on the following pages, the background in which Freud worked during these years although the papers published in this period might, in themselves, not deserve so much space.

The Physiological Institute was miserably housed in the second story and basement of a dark and smelly old gun factory. (16) It consisted of a large auditorium and of two rooms—one of them being Brücke’s office—with two windows each. The microscopes for the freshmen students had their place in the auditorium. Further, there were a few small cubicles, some without any light, serving as electro-physiological, chemical and optical laboratories. Of those, some adjoined the auditorium and Brücke’s office in the second story. Others were in the basement. The animals were kept in a shed in the yard. There was no gas and no water. All heating had to be done over a spirit lamp and the water was brought up from a well in the yard. This was the job of the janitor who carried one bucketful up the two stories

every morning and deposited it in the large room in which he doubled as the mechanic and which he shared with Brücke's two assistants, the professors Fleischl and Exner, and with their famuli. Yet, this institute was the pride of the medical school due to the number and distinction of its foreign visitors and students.

In fact, Brücke's Institute was an important part indeed of that far-reaching scientific movement best known as Helmholtz' School of Medicine. The amazing story of this scientific school started in the early forties with the friendship of Emil Du Bois-Reymond (1818-1896) and Ernst Brücke (1819-1892) soon joined by Hermann Helmholtz (1821-1894) and Carl Ludwig (1816-1895). From its very beginning this group was driven forward by a veritable crusading spirit. In 1842 Du Bois wrote: "Brücke and I pledged a solemn oath to put in power this truth: 'No other forces than the common physical chemical ones are active within the organism. In those cases which cannot at the time be explained by these forces one has either to find the specific way or form of their action by means of the physical mathematical method or to assume new forces equal in dignity to the chemical physical forces inherent in matter, reducible to the force of attraction and repulsion.'" (17)

These men formed a small private club which in 1845 they enlarged to the *Berliner Physikalische Gesellschaft*. Most of its members were young students of Johannes Müller—physicists and physiologists, banded together to destroy, once and for all, vitalism, the fundamental belief of their admired master. Strangely enough, Johannes Müller did not mind. On July 23, 1847, at the meeting of this society, Helmholtz read a paper on the principle of the conservation of energy—with the modest purpose of giving a sound foundation to the new physiology. Thus, casually, started the career of one of the leading physicists of the century. Du Bois, Brücke, Helmholtz and Ludwig remained lifelong friends. Within twenty-five or thirty years they achieved complete domination over the thinking of the Ger-

man physiologists and medical teachers, gave intensive stimulus to science everywhere, and solved some of the old puzzles forever. As for vitalism—they lived long enough to see it rise again in 1890. However, in the seventies they and their physiology were a power not yet seriously challenged.

Brücke, whom in Berlin they called “Our Ambassador to the Far East”, kept, in his Viennese classes, very close to his elaborate notes. These, in 1874, he published as *Lectures in Physiology*. The first forty pages contain, in substance, the general ideas of the physicalistic physiology which captivated the student Freud.

Very briefly they are: Physiology is the science of organisms as such. Organisms differ from dead material wholes in action—machines—in possessing the faculty of assimilation but they are all the phenomena of the physical world; systems of atoms, moved by forces, according to the principle of conservation of energy formulated by Helmholtz; the sum of forces (motive forces and potential forces) remains constant in every isolated system. The real causes are symbolized in science by the word “force”. The less we know about them, the more kinds of forces do we distinguish; mechanical, electrical, magnetic forces, light, heat. Progress in knowledge reduces them to two—attraction and repulsion. This applies as well to the organism man. Contrary to Descartes, one cannot believe that the perpetual changes which we experience and which happen to our ego are not the effect of external causes. Brücke then turns to an elaborate presentation in two volumes of what was then known about the transformation and interplay of physical forces in the living organism. I do not know how better to describe the spirit and content of Brücke’s lectures than with the words which Freud used in 1929 to characterize psychoanalysis from its dynamic standpoint: “The forces assist or inhibit one another, combine with one another, enter into compromises with one another, etc.”

Very closely connected with this dynamic aspect of Brücke’s physiology was its evolutionistic orientation. The

organism is not only a part of the physical universe, but the organismic world itself is one family. Its apparent diversity is the result of divergent developments which started with the microscopic unicellular "elementary organisms". It includes plants, lower and highest animals, as well as man, from the hordes of the anthropoids to the peak of his contemporary western civilization. In this evolution of life, no spirits, essences, or entelechias, no superior plans or ultimate purposes are at work. But the physical energies alone cause effects—somehow. Darwin had shown that there was hope of achieving in a near future some concrete insight into this "How" of evolution. The enthusiasts were convinced that Darwin had shown more than that—in fact had already told the full story. While the sceptics and the enthusiasts fought with each other, the active researchers were busy and happy to put together the family trees of the organisms, closing gaps, rearranging the taxonomic systems of plants and animals according to genetic relationships, discovering transformation series, finding behind the manifest diversities the homologous identities.

This physiology was a part of the general trend of western civilization. Slowly, continuously, it had risen and grown everywhere through the preceding two or three hundred years, steadily gaining momentum from the end of the eighteenth century and increasing rapidly in velocity and expansion after the eighteen thirties. This trend, weaker in Germany than in England and France, was interrupted there from about 1794 to 1830 (from the great to the little French revolution) by the period of *Naturphilosophie* (philosophy of nature). (18)

Naturphilosophie is the name of the pantheistic monism, close to mysticism, which, professed by Schelling—repeated, developed and varied by a host of writers—was eagerly accepted by the average educated man and literary lady. The Universe, Nature, is one vast organism, ultimately consisting of forces, of activities, of creations, of emergencies—all these—organized in eternal basic conflicts, in polarity; reason,

conscious life, mind, being only the reflection, the emanation, of this unconscious turmoil. These ideas have been expressed before and since and contain the seeds of some of the scientific theories of the nineteenth century and of our time. But it is not the ideas which are characteristic of the movement nor the romantic temper which envelopes them. This was a general European trend. What characterizes the *German Naturphilosophie* is the aspiration expressed in the name "speculative physics" (which Schelling himself gave to his endeavors) and the unbalanced, megalomaniac emotionalism of the fantasy and of the style of these writers. Fechner praised "the gigantic audacity" of Oken, a prominent representative, while a sober English historian puts it thus: "They exhibit tendencies that seem foreign to the course of European thought; they recall the vague spaciousness of the East and its reflection in the semi-oriental Alexandria".

Physicalistic physiology—although not by itself—overthrew philosophy and took its place. As has happened before, the conqueror introjected the emotionalism of his victim. "Unity of science", "science", "physical forces" were not merely directing ideas or hypotheses of scientific endeavor; they became almost objects of worship. They were more than methods of research—they became a *Weltanschauung*. The intensity of this temper varied with scientist to scientist; from place to place. In Berlin with Du Bois-Raymond it was at the maximum, strangely mixed with Prussian nationalism. In Austria, *Naturphilosophie* never had much power, therefore the physiology-fanaticism was at a minimum in Vienna and with Brücke. Yet it was there.

Brücke's writings cover a long span of time and a wide variety of topics. They begin in 1841 with the physiology of stereoscopic phenomena and end in 1892 with a pamphlet on how to protect life and health of one's children. Among them are classical pieces of research on the movements of *mimosa pudica*, the color change of chameleons, the structure of the "elementary organism", the biochemistry of urine, while the bulk—well over one hundred and twenty books and pa-

pers—were of more or less transitory importance only. He himself used to say: “A scientific truth lasts five years at most”. Amongst these papers are many which, in terms of physicalistic physiology, deal with problems of psychology and social psychology: seeing, hearing, language, poetry and art. The following list of his publications during the six years in question gives only a faint impression of this variety:

1. The Sources of Ammonia in Distilled Water. (1876)
2. Suggestions Concerning Improvement of Drinking Water Through Heating. (1876)
3. The Absorption Spectra of Potassium Permanganate and its Uses in Quantative Analysis. (1876).
4. A Contribution to Thermo-dynamics. (1877).
5. Fragments of a Theory of the Formative Arts. (1877)
6. Voluntary Movements and Cramps. (1877)
7. The Schistoskop. (1877)
8. Some Sensations Belonging in the Field of the Optical Nerves. (1878)
9. The Relationship between the Formation of Spontaneous Oil Emulsions and of the So-called Myelin Sheah. (1879)
10. Some Consequences of the Young-Helmholtz Theory. (1879)
11. Training in the Classical Languages is Necessary for Physicians. (1879)
12. The Metric Accentuation in Verses. (1879)
13. } Nitrogen and Sulpha-containing Non-cristalizable Acid
14. } Obtained by Treatment of Chicken Protein with Potassium Permanganate. (1881)
15. Action in Painting and Sculpture. (1887)
16. The Determination of Urea with Oxalic Acid. (1881)

Brücke preferred that the student presented his own plans and projects but he was quite ready to formulate a problem for those beginners who were too timid or too vague in their interests. Freud belonged in the latter group when he entered the Institute as famulus (which is about the equivalent of a postgraduate research fellow) in 1876—probably in the fall, on his second return from Trieste. Brücke

set him behind the microscope on work concerned with the histology of the nerve cells. This topic obviously was part of Brücke's great interest in "psychology".

Freud formulated, a few years later, the general situation as he found it in this field in the following words: "Very soon after the recognition of the nerve cells and of the nerve fibres as the fundamental parts of the nervous system began the efforts to clarify the finer structure of these two elements, motivated by the hope of using the knowledge of their structure for the understanding of their function. As is well known, up to now neither sufficient insight nor agreement has been reached in either of these two directions. One author thinks of the nerve cell as granulated, the other as fibrilose; one thinks of the nerve fibre as a bunch of fibriles but another as a liquid column. Consequently while one elevates the nerve cell to the basic source of nervous activity another degrades it to a mere nucleus of the Schwann sheaths". (19)

Together with the problem of the structure of the nervous elements goes the interesting question of whether the nervous system of the higher animals, at least of the vertebratae, is composed of elements different from the nervous system of the lower animals; or whether the simple and the complicated systems alike are built of the same units. This topic was highly controversial at that time. The philosophical and religious implications seemed to be very disturbing. Are the differences in the mind of higher and lower animals only a matter of degree of complication? Does the human mind differ from that of some molluscs—not basically but correlative to the number of the nerve cells in both and the complication of their respective fibres? Scientists were searching for the answers to such questions in the hope of gaining definite decisions—in one way or another—on the nature of man, the existence of God and the aim of life.

Into this vast and exciting field of research belonged the very modest problem which Brücke put before Freud. In the spinal cord of the Amoecetes (*Petromyzon*), a genus of fish belonging to the primitive Cyclostomatae, Reissner

had discovered a peculiar kind of large cell. The nature of these cells and their connection with the spinal system elicited a number of unsuccessful investigations. Brücke wished to see the histology of these cells clarified. After a few weeks Freud came up with the quite unexpected discovery that the roots of the posterior nerves originated in some of these Reissner cells. Although this find did not explain the nature of the cells, it did promise a simple solution and eliminated the various hypotheses current in the literature. Brücke, it seems, thought that this was good enough for a beginner, and pressed for publication. Freud obliged by hurriedly putting together a report. (20) His dissatisfaction with the unfinished work, however, is noticeable in many places in the paper. In style and organization it is far below the paper on the eels and of the succeeding publications of his student years. Brücke filed the study with the Academy of Science at its meeting of January 4, 1877. It appeared in the January bulletin of the Academy.

Freud continued on his thorough investigation of the Reissner cells, and published a second report on Petromyzon in July of the following year. (21) Here he assembled an amazingly complete bibliography—eighteen pages of his report deal with the literature. This historical conscientiousness was not quite favorable to the young scientist's ambitions: "I must accuse myself of having falsely thought that I was the first one to describe—based on direct and certain observations—, the origin of the posterior nerve roots in certain cells of the petromyzon. Only shortly after the publication of my paper did I find in Stieda's abstracts of the Russian literature an abstract of a paper by Kutschin which contains important information on the origin of the posterior root. Due to the friendliness of Professor Stieda in Dorpat, who had sent me the Russian paper, I could examine the pictures by Kutschin and satisfy myself that Kutschin had seen, already in 1863 in his preparations, convincing proof of the origin of the posterior roots in the posterior cells. By way of apology I can only say that

Kutschin's statements—perhaps because his pictures were not available to the German histologists—were quite generally overlooked". Thus was not Brücke wrong after all, to insist on the publication of the preliminary paper?

Aided by an improvement in the technique of the preparation, Freud established definitely that the Reissner cells "are nothing else than spinal ganglion cells which, in these low vertebratae, where the migration of the embryonic neural tube to the periphery is not yet completed, remain within the spinal cord." (21) "These scattered cells mark the way which the spinal ganglion cells have made throughout their evolution." (13) This solution of the problem of these cells is a triumph of precise observation and genetic interpretation—one of the thousands of such small achievements which have finally established among scientists the conviction of the evolutionary unity of all organisms.

But Freud made even a major discovery on *Petromyzon*: "The spinal ganglion cells of the fish were known for a long time to be bipolar (possessing two processes) while those of the higher vertebratae are unipolar." This gap between higher and lower animals Freud has closed. "The nerve cells of *Petromyzon* show all transitions from uni- to bipolarity including bipolars with T-branching". This paper, in content, presentation, and implication is without any doubt well above the beginner's level. Brücke filed it with the Academy on July 18, 1878 and it appeared in its Bulletin, eighty-six pages long, the next month.

The same general problem is the aim of Freud's next investigation which he conducted by his own choice in the summer months of 1879 and 1881. This time the objects are the nerve cells of the crayfish. Here he examines the live tissues microscopically—a technique which, at that time, was as yet very little used, undeveloped and difficult—and he reaches the definite conclusion that the nerve fibres have without exception fibrillose structure. He recognizes that the ganglion consists of two substances, of which one is net-like, and the origin of the nerve processi. This study, (22) which

Freud himself filed with the Academy of Sciences at the meeting of December 15, 1881 and which appeared in the Bulletin of the Academy in January 1882, excels in the choice of its method, the exacting care given to its development, the caution shown in the argumentation, the direct approach to the key problem as well as in its precise, definite and significant results.

With this paper and the two preceding ones Freud has done his share to pave the way for the neuron theory. One might safely go even a little further and claim, as did Brun (3) and Jeliffe, (6) that Freud had early and clearly conceived the nerve cells and the fibrils to be one morphological and physiological unit—the later neurones. In his research papers he confined himself strictly to the anatomical viewpoint, although he makes it clear that his investigations were conducted with the hope of gaining insight into the mystery of nerve action. Only once, in a lecture on “the structure of the elements of the nervous system” (23) which summarizes his work, does he venture into this land beyond histology with the one paragraph: “If we assume that the fibrils of the nerve fibre have the function of isolated conductive pathways then we may assume that the pathways which are separated in the nerve fibre are confluent in the nerve cell; then the nerve cell becomes the beginning of all those nerve fibres which are anatomically connected to it. I would transgress the limitations which I have imposed on this paper were I to assemble the facts which are in favor of that assumption; I know that the existing material is not sufficient for a decision on this important physiological problem; yet if that assumption could be proved we would take a great step in the physiology of the elements of the nerve system. Then we could consider the possibility that the nerve as a unit conducts the excitation”.

This lecture Freud delivered at the psychiatric society—within a year after he left the Brücke Institute—in 1882 or 1883. It was published in the *Jahrbücher für Psychiatrie* early in 1884. Here he gives to a broad audience of physi-

cians—not to specialists in nerve histology—an account of the general problem situation in which his highly specialized investigation originated. He details his methods and his findings and in a few sentences he intimates the far reaching vistas opened by his results. We find here the same caution and boldness, the same style of argumentation which characterizes the many accounts of his findings in psychoanalysis which Freud later gave to audiences unfamiliar with the goals, methods and experiences of the specialist. The first lecture of this kind shares with its successors the condensation of complex nets of facts and of complicated chains of thought in a few simple and lucid sentences. But in contrast to them this lecture contains sharp criticism of opponents. Although in controlled language, they are quite out of keeping with his previous and later characteristic aloofness.

Amongst his victims is Fleischl, his friend and teacher in the Brücke Institute. He dissects and rejects a study of Fleischl's on the structure of the fibres, though in gentle words, but thoroughly, resorting even to the method of the agonistic use of psychological interpretation; pointing out what the psychological motives of the observer might be, which lead him to an erroneous foundation for his findings. One wonders whether the dissatisfaction and frustration caused by his leaving the institute did not break through his usual contained literary attitude.

Should this be true or not—the polemic against Fleischl serves us as a reminder that the anticipation of the neuron theory with which we credit Freud was not implied in the teaching of Brücke and his staff. Although this theory is in the spirit of their teaching, neither Brücke nor Fleischl nor probably Exner and Paneth had at that time directed their thoughts in this direction. It seems they were Freud's own. Still it ought to be stressed that Freud had no part in the actual development of the neuron theory. His histological papers were noticed and occasionally quoted by some neuro-anatomist. They certainly served to create for him the reputation of a coming young man but they had hardly any

influence on the course of research and theory. His physiological ideas condensed into one little paragraph hidden away in a popular lecture to psychiatrists most certainly was not even noticed. It had to wait for a friendly biographer to be discovered.

III. NEW METHODS.

Freud's success in the histology of the nerve cells was greatly facilitated, if not made possible, by an improvement in technique on which he hit in 1877, soon after he entered the Institute of Physiology. He writes in a brief "Note on a Method for the Anatomical Preparation of the Central Nervous System" dated May 26, 1879: (24) "I use Reichert's mixture as I have modified it for the purpose of preparing in a guaranteed and easy way, the central and peripheral nervous system of the higher vertebratae (mice, rabbits, cattle) . . . I have tried the method with the cerebral nerves of infants—Professor Dr. E. Zukerkandl kindly participating. We have found that it considerably facilitates the preparation of nerves situated in the bone channels and in the preparation and disentanglement of anastomoses and nerve nets. . . Furthermore, I used it successfully for the preparation of phlegm and perspiration glands, pacini bodies, hair-roots, etc."

This is evidence of the scope of Freud's studies which surpassed the problem on which, on Brücke's suggestion, he worked at that time. The new technique, moreover, helped him in his days as a "demonstrator" at the Institute of Physiology. The equivalent of a teaching assistant, this position required him to prepare the anatomical specimens and histological slides for the classes of Brücke and his assistants.

Freud's modification of the Reichert formula prescribes the mixture of one part of concentrated nitric acid, three parts of water and one part of concentrated glycerine. It seems that nobody outside the institute gave any attention whatsoever to this invention. In fact to call it an invention—

although logically correct—may sound like idolatry, a weakness quite common to biographers of great men. However to Freud this modest achievement was the first realization of a high ambition. Six years later he returns with a second effort to this field.

“Innumerable methods were devised by histologists which proved themselves useful in the hands of their inventors only—this is why I have decided to publish even the pettiest directions” of a “new histological method for the study of nerve tracts in the brain and spinal cord”. (25) This method Freud had developed in the fall of 1883. At that time he had left the institute of Brücke, prepared himself for private practice and took time out for research in Meynert’s Institute of Brain Anatomy.

Freud was convinced of the usefulness of this new method. He praises the “wonderfully clear and precise picture” which one receives if one carefully follows his way of dyeing the brain preparation with gold chloride. The results achieved were far superior to any other dye technique known at that time and he was satisfied with its complete reliability. No longer does he speak modestly of having “hit on it”. This method he had laboriously and successfully developed in many experiments following a hint which Flechsig had published in 1876 but had not, himself, followed through.

This time, so it appears, Freud was determined to carry the day. He published a brief sketch, (26) as histologists usually do, but in order to escape “the fate of other inventors” as he says and of his own first trial six years previously—as one may assume—he followed up this publication with the detailed seven-page presentation (25) which contains the lines quoted above. Not satisfied with this he writes a third version—this time in English—and published it in “Brain”. (27) These efforts brought him some success. This invention was not completely overlooked. Some students, off and on, have used it and at least one of them, in one American journal, still remembered the method in 1888. (28) However, it was not the gold chloride preparation of

nerve tracts which became known as the Freudian method.

These two new methods and their fate would be of no importance were it not that they complete the picture of the young scientist Freud. It is a picture that has a striking likeness to that of the inventor of the psychoanalytic method. For Freud, as he has many times emphasized, psychoanalysis is first of all a new technique by which a whole realm of facts, inaccessible before, can be brought to light. It is a new instrument of observation, a new tool of research. In the second place only is it a body of new knowledge gained by the use of the new instrument. The Freudian discoveries are the almost incidental results of the Freudian invention. From his early scientific days on, his central aspiration was, so it appears, to do more than to collect and to marshal facts already known; more than to add a few units to the army against the dark and the unknown. He longed to provide it with a new type of weapon—an achievement which, with one magic stroke might multiply its fighting power.

Whether or not these metaphors which try to establish some continuity from Freud's early day dreams to his life work have any validity or are just a matter of style I do not know. Yet I want to stress emphatically that Freud's persistent interest in the invention of methods, though due to the individual trend of his mind, coincides with the basic ideas of the Brücke Institute and with the logical structure of science. Scientific progress runs from a new instrument to a new body of facts. The invention of the microscope, for instance, preceded histology. And in the history of any limited scientific field only new instruments and techniques can, in the long run, bring new facts. From there science proceeds to a new theory: the organization of the new and the old knowledge into one body of facts; and from the theory it finally runs to "speculation"—that is to the guessing at questions and answers beyond existing means of observation. It is very rare when one and the same man is productive in several of these phases, and almost never does it happen that he is equally effective in all. Psychoanalysis

is an example of this rarest case: Freud invented the instrument, used it for a great number of discoveries, provided the organizing theory and the speculation beyond the known. The remarkable fact is, that he had already reached out for such encyclopedic achievement in his twenties. Freud's lecture on "The Structure of the Elements of the Nervous System", (23) delivered at the Psychiatric Society in 1882, presents the new technique, the new findings due to it, the theory adequate to them and some glances beyond. Every Freudian essential is there—in nucleo—but already sharply defined.

IV. PHYSIOLOGY

Commenting on his professional education, Freud remarked that the physiology of his student years "was far too much concerned with histology". (1) This mild reproach stands out sharply against the background of the superlative praise with which Freud usually spoke of Brücke and his school. Moreover, among all the possible objections to Brücke's teaching this one is the least justified. True, in Brücke's Institute the microscopic and experimental approaches were still not separated in the seventies. Physiological experimentation, including the biophysics and biochemistry of today, became at that time increasingly the *via regia*, and some physiologists indulged in contempt of the microscopists. Not so Brücke. He continued to announce his classes in the lingo of the Vienna University as "Physiology and Higher Anatomy". To him the knowledge of the spatial organismic structure seemed as necessary as the knowledge of the forces playing on this apparatus, changing or reproducing it. The structure can be revealed by the microscope only. In Brücke's mind there was no opposition between anatomy and physiology; between microscope and experiment. This was the attitude which had already made famous his first major work in 1847. Yet in Freud's time the work done by Brücke and his assistants Fleischl and Exner was, in fact, almost completely physiological in the

narrow sense of the word, dealing with organismic function and using animal experiment as one, though not as the only method. There were few institutes in Europe where one could learn physiology equally well.

We have no indication that Freud made use of this opportunity. Considering the full freedom which existed in Brücke's Institute it is quite unlikely that any kind of external pressure kept him behind the microscope after he had finished his first histological assignment on the Reissner cell in 1878. In 1883, shortly after Freud left the Institute to prepare for medical practice, he again took up research. Yet even then—although undoubtedly free to choose topic and method—he returned to anatomical investigations. Only when clinical neurology took more and more of his increasingly fewer spare hours he discontinued all anatomical-histological research. His work in neurology Freud did not consider to be scientific research at all, in spite of its impressive quantity and the unanimous recognition which it found. Only in the middle nineties, when, as a cathartic psychoanalyst he again found himself behind an observation object, studying the structure of the mind, hoping for insight into the workings of the brain, did he feel that he had returned to science and enjoy this fact "as the triumph of his life". Thus it might be concluded that his heart simply was in histology, and that physiology did not appeal to him. However he stressed too frequently and too seriously the subordinate character of the study of forms, for the understanding of the function—guessing the drama from the stage setting, one might say. From the beginning of his scientific career, the knowledge of the acting forces certainly was a cherished goal, but for many years, like Moses, he stood before the forbidden promised-land with only a guess of what it might look like.

A fact not mentioned in Freud's autobiography and overlooked by his biographers puts this conflict into sharp relief. Freud did make several efforts in the field of physiology proper during his student years, but not in Brücke's

Institute. At that time a great deal of physiological research was done under the guidance of Stricker. Solomon Stricker, a contemporary of Claus (born 1834), trained by Brücke, had been Professor Ordinarius and chief of the Pathological Institute since 1873. (29) His early reputation was derived from embryological studies. His later work was concerned with the physiology of the vascular system and with the theory of consciousness, speech and thought. He is credited with transforming pathology from an anatomical into an experimental physiological discipline. In his institute a large amount of meritorious work was accomplished in various fields of physiology. His assistants were good men, but very few great talents developed in his school. His vanity, quarrelsomeness, righteousness and some personal and scientific eccentricities were at fault—so it was gossiped in Vienna at that time. Freud worked in this institute at least twice; once in 1878 and again in 1883 to 1884.

At the meeting of the Medical Society in Vienna on October 17, 1879 Stricker introduced his paper on *Azinous Glands* with the statement that his student Freud had, at his suggestion, conducted experiments on this topic for a period of half a year, but had accomplished nothing. After Freud's failure Stricker collaborated with Spina and obtained interesting results. (30) Allowing half a year for these new experiments, Freud's efforts must have started sometime in the second half of '78, at the latest.

Thus Freud had tried his hand in experimental physiology soon after he had completed the histology of the Reissner cells in Petromyzon. He failed. Immediately afterwards he returned, by his own choice, to Brücke's Physiological Institute. Here he did not take up physiology but he went back to the microscope and started work on the *Nerve Cells of the Crayfish*, using the live-tissue method of which Stricker and not Brücke was the protagonist in Vienna.

In 1883, after he had left Brücke, we find him again in Stricker's Institute. (31) There he participated, together with Wagner-Jauregg, Gaertner, Spina and Koller, in ani-

mal experiments as part of a research project on the function of glands and of the circulatory system. Again Freud accomplished nothing. Simultaneously he had started research in brain anatomy and worked on his second invention—the gold-chloride method. The resumption of physiological research, it seems, was only half hearted but it indicates that his urge to go into physiology proper was still alive. Unlike Moses, he tried to penetrate the promised land but was forced back on every attempt. Not the lack of facilities, of opportunities, of teachers or of stimulation frustrated him. And certainly there was no lack of interest. Instead the ability for physiological work was missing. This can be said on the negative evidence that no physiological achievements of his are extant. There is even one positive clue: Freud has published, in 1885, a single piece of experimental work—the effect of cocaine, measured by the dynamometer. (32) It is a very poor effort indeed. In concept and technique it is oversimplified, uncertain and uncritical—the work of a beginner with little promise; quite different from the qualities of his initial histological work. Not, as he said, zoology, but physiology was really the field in which “the peculiarities and limitations of his gifts denied him all success”.

Thanks to Freud, such “gifts” are no longer the last entities to psychological understanding. Beyond them exist determinants of “peculiarities and limitations”. As in the case of Freud's alleged failure in zoology we might guess at one or the other reason for his suppressed failure in physiology. Stricker was, even less than Claus, a teacher whom Freud “could respect”. One can see very well why he had not succeeded with Stricker. But why had he not grasped the opportunities at the Brücke Institute? Why had he accepted Brücke as authority and model only in the investigation of the setting and not of the drama? Brücke had started him on the dissection of the dead body. Had Freud unconsciously taken this advice to mean that Brücke had exiled him to the preliminary lowlier study of the structure and had reserved for himself and the older members of

the Institute the higher wisdom about the workings of the living organism? Had he thus reaffirmed Freud's father's angry scolding of the child "when he, driven by early sexual curiosity, had intruded into the parental bedroom"? (15) And had he therefore tabooed physiology? Perhaps.

One feels on safe ground in pointing to a more superficial but probably concomitant factor. The animal experiment is a far more brutal exercise of power over the rights and life of the creature than the investigation of the corpse. And life cells of the crayfish?—but are they not "dead" compared with living guinea pigs, rabbits and dogs? As an adolescent Freud retreated from the power over man into the science of nature. The same basic design will reappear when Freud in his middle-thirties gives up hypnosis in search for "a less coarsely interfering" method. These were the two turning points in Freud's relation to science; at the first he became a scientist; at the second, he invented psychoanalysis. And in between these two marks he stayed away from experimental physiological activity or, after brief excursions, returned to the more subtle exercise of power, to the role of observer of mere structure.

V. TRANSLATIONS.

To Freud's university years belongs the only work ever published by him which has no connection with his scientific or therapeutic interests. In 1879 Freud did a German translation of some essays of John Stuart Mill. The editor of Mill's collected writings in German was Theodore Gomperz, a philosopher and historian of high standing in the university and in the society of Vienna. Freud substituted for Eduard Wessel, the young translator who had died suddenly during the preparation of the twelfth volume. He started the work in the fall of 1879 and completed it in December of that year.

Why Freud accepted this commission is not known. He was at that time on involuntary leave from science, serving his one-year term in the army, which was compulsory for

all physically able students. He was no model soldier it seems; he recalls gleefully how he spent his twenty-first birthday, May 6, 1880, under arrest. I can imagine that he seized the opportunity to kill the boredom of the barracks and to forget the discomforts of garrison life, by mental exertion—a kind of relaxation which has a touch of bravado indeed, considering the physical, psychological and moral strain of the service. Furthermore, even a modest translator's fee must have been quite welcome, in this year especially.

However, the task may have interested Freud beyond such secondary motivations. When Freud decided to take his place among the scientists and not with the politicians he had by no means abandoned interest in, and curiosity for social questions. Three of the four essays by Mill which he translated deal with the labor question, the enfranchisement of women and socialism. Freud, in his later years, heartily abhorred philosophy and it is not likely that he ever had much interest in it. But Mill's philosophical work is in distinct contrast to the metaphysical systems which were specifically called "philosophy". Mill's work was very close to the empirical physicalistic spirit of the Brücke Institute. It is quite possible that Freud was attracted by the topics of the essays and by the writer as well. And it is certain that he liked to translate. Freud loved languages and writing. He read Greek and Latin for pleasure in his high school years. He had an early command of English and French and later wrote several papers in these languages. He did a considerable amount of translating during his life—two volumes of Bernheim and two of Charcot, though on these occasions, even more than with Mill, secondary determinations existed. Freud as a translator was so careful, so brilliant and so rapid that translating, as such, must have appealed to him as a challenging pastime.

When Theodore Gomperz' son Heinrich, himself a philosopher and historian, prepared the biography of his father he asked Freud how he became the translator of the twelfth volume. Freud replied, in a letter dated June 9, 1932 (in

translation) : "I know that I was recommended to your father by Franz Brentano. Your father at a party . . . mentioned that he was looking for a translator and Brentano, whose student I then was or had been at a still earlier time, named my name". (34) That he had personally known Brentano; that he once had been his student and was well remembered by him, seems strange. Franz Brentano has not published much, and his teaching in philosophy and psychology did not create a great stir during his lifetime. (35) But Husserl's phenomenology and the various shades of logic and psychology ("Gegenstands-theorie") of Meinong, Marty and others, trace their origins to him. Several newer trends in psychology like the schools of Stumpf and more recently that of the Gestalt psychology, acknowledged him as one of their distinguished forerunners. In fact he had, in 1870, turned from metaphysics and physiological physicalism alike and developed psychology as a science based on empirical observation of the consciously "given". One is inclined to think of Brentano and Freud as almost diametrical opposites.

Heinrich Gomperz comments on the relation between Freud and Brentano which he feels is "not quite insignificant: We ought to remember that Freud had always opposed the more or less materialistic medicine of his time, stressing the relative independence of the 'psychic apparatus' from the physical, and in this connection maintained that it is possible to influence psychical maladies psychically. May we speak, perhaps, of a certain after-effect of the influence of a psychologist, who, more than any other, distinguished between 'physical' and 'psychic' phenomena and erected his whole doctrine on the basis of this distinction?"

That Gomperz misinterprets Freud's position follows clearly, if there were any doubt, from the preceding chapters of the present paper.

It is impossible that Freud at that time, or at any time for that matter, was a follower of Brentano. One even wonders whether he would have cared to understand the finer points of his arguments. This does not exclude the possibility that

Freud was impressed by some of Brentano's polemics and statements, that he preserved them in his preconscious and that they influenced his thoughts twenty years later when he, disappointed in the existing psychological theories, ventured into this broad field on his own. Brentano's classification of the mental phenomena (perception, judgment and love-hate); his ideas concerning genius; his determinism, and—in some complex way—his emphasis on the fact that all psychological phenomena refer to an object (intentionalism)—to put it crudely—all these thoughts could have had a belated influence on Freud in the nineties. So could have, as T. H. Merlan points out, Brentano's thorough historical presentation and most serious consideration of the doctrine "of the unconscious" in spite of Brentano's rejection of the concept of unconscious psychic activity. All these could have—if Freud had ever been a student of Brentano. In his letter to Gomperz Freud states that he had been a "Hoerer" of Brentano which means that he had "attended his lectures"; literally that he was one of Brentano's "listeners" rather than one of his pupils. Brentano was a very famous personality in the academic Vienna of his time and his lectures were crowded not only by students but by visitors and academic notables as well. Yet very few of his "Hoerer" came to study his philosophy and psychology.

Brentano held the attention of all Vienna from the moment he arrived from Würzburg as a professor of philosophy in 1874. His very name made him interesting. A nephew of the famous romantic poet Clemence Brentano, a grandson of Sophie La Roche, the friend of Goethe's youth, a nephew of Bettina, the famous addressee of Goethe's "Correspondence with a Child,"—he was welcomed in the literary circles and salons. But more exciting than the history of his family was his own. A doctor of philosophy at the age of twenty-four, he decided to study theology and was ordained two years later as a Catholic priest. At thirty-two he courageously led the fight against the Pope's intention to set up the dogma of infallibility. Failing in his efforts, he

defrocked himself and resigned his professorship in Würzburg. The Viennese liberal scientists acclaimed his appointment and soon found out that his personality, in sincerity, courage and charm, matched his pedigree and his spectacular action. Just at the time when Gomperz was looking for a translator, Brentano offered Vienna another exciting spectacle. He wanted to marry Ida Lieben, "one of the most noble daughters of Vienna," but the reactionary interpretation of an old Austrian law made such a marriage illegal for a former priest. Brentano resigned his position, acquired Saxon citizenship and finally married in Leipzig on September 16, 1880. He returned to Vienna to resume his lectures at the university—this time a simple lecturer (Privat-dozent).

That Freud was interested in Brentano and respected him as a man and a fighter there can be no doubt. Yet I have no clue to the understanding of Brentano's interest in Freud. However, the recommendation of a young student as a translator for some rather unphilosophical essays by Mill does not necessarily indicate a high esteem for him. The assignment certainly did not require adherence to Brentano's teachings. It was more important to find someone who knew English. That Freud excelled in Brentano's seminar with his linguistic knowledge is possible. But it is equally possible that Brentano might not have been impressed by Freud at all—might hardly have remembered him personally—but was following the suggestion of one of their mutual friends. Fleischl, Exner, and Freud's close friend, Paneth, were personally and through their families, well acquainted with Brentano; Joseph Breuer was his family physician. At any rate, since we do not know how close Freud's acquaintance with Breuer and Paneth was in 1879, the reconstruction presented is hypothetical.

Horace Gray, in his list of Freud's 65 pre-analytic writings, makes a subjective comment on only one, the Stuart Mill translation. "In a footnote to the German version the editor Gomperz tells us (1) that the author inserted in the reprint of the essay

a short preface, in which he explains that by far the greatest part of it is the work of his wife, since dead in 1858, highly valued by him for her preeminent qualities of mind and character; and (2) that he publishes no translation of Mill's later related work *The Subjection of Women*, 1869, which had been translated as *Die Hoerigkeit der Frauen*. The above facts are interesting in connection with Freud's later comment: 'That hostile embitterment displayed by women against men, never entirely absent in the relation between the sexes, the clearest indications of which are to be found in the writings and ambitions of emancipated women'.—In passing we note the high quality of the German translation of the *Enfranchisement* in its close adherence to the original without sacrificing smoothness. A curious point is the spelling of the translator's name as Siegmund, both on the title page and in the editor's epilogue.'" (4)

The influence of English philosophy, literature and political thought on Freud is an interesting topic which deserves a separate study. As every one who mentions Freud's translation feels provoked to comment on it I also want to make a remark. In a conversation about Plato Freud admitted in 1933, that his knowledge of Plato's philosophy was very fragmentary but that he had been greatly impressed by his theory of anamnesis and that he had, at one time, given it a great deal of thought (36). Amongst the essays in the twelfth volume, Stuart Mill's paper on "Grote's Plato" takes a conspicuous place. Mills presentation treats the theory of reminiscence with sympathy and in general is a forceful debunking of the views on Plato's philosophy which high school teachers were accustomed to preach at that time. Mill's common sense must have appealed to Freud very much and this essay could well be the main source of Freud's "fragmentary knowledge".

2835 Broderick St.
San Francisco 23, Calif.

NOTES

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