

GERM THEORY, HYSTERIA, AND FREUD'S EARLY WORK IN PSYCHOPATHOLOGY

by

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THE PREOCCUPATION of the late nineteenth-century medical researchers with germ theory has been generally acknowledged. "With the work of Pasteur and Koch, . . . there penetrated rapidly into all fields of medicine the idea that infinitely small beings, endowed with special pathogenic qualities, played a pre-eminent role in producing many diseases. The new concept made such a great impression that for a while it was believed that the cause of all diseases could be ascribed to microbes alone Almost completely dominant, bacteriology at this period became the centre and goal of medical investigation."¹ However, accepting germ theory involved much more than simply discovering that various diseases were caused by specific micro-organisms. Its adoption entailed fundamental changes in the concept of disease, in approaches to nosology and diagnosis, and in standards of explanation in medical science. If we adopt this broader perspective, our current understanding of vitamin deficiency diseases can be seen as a direct consequence of the changes resulting from the adoption of germ theory.² In this essay I will argue that Freud's early work in psychopathology constitutes another ramification of the basic research strategy of germ theory.

The theoretical connexions between germ theory and Freud's early work seem to have been entirely ignored. To take one prominent example, a great part of Ellenberger's *The discovery of the unconscious* is "devoted to authors and systems of thought, which . . . could be called sources or precursors of Freud." In a twelve-page summary he gives "a succinct list of these sources, insofar as they are known today." The list includes more than two dozen persons and movements but germ theory is not mentioned.³ By ignoring the connexion between Freud's work and germ theory, one overlooks those aspects of Freud's work that show it to be *both* fundamentally different from the work of nearly all his immediate predecessors who wrote on psychopathology, *and* fundamentally allied to work that was being carried out on infectious diseases.

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¹ Arturo Castiglioni, *A history of medicine*, 2nd ed., trans. by E. B. Krumbhaar, New York, Alfred A. Knopf, 1947, p. 809.

² K. Codell Carter, 'The germ theory, beriberi, and the deficiency theory of disease', *Med. Hist.*, 1977, 21: 119-136.

³ Henri Ellenberger, *The discovery of the unconscious*, New York, Basic Books, 1970, pp. 534-546. Hannah Decker, Ernest Jones, Ola Andersson, and Siegfried Bernfield also fail to mention germ theory in their discussions of the development of Freud's doctrines.

I

In the early nineteenth century, diseases were frequently identified by reference to a particular morbid change in a particular organ, such as inflammation of the lungs, or softening of the brain. When this was not possible, and even in many cases when it was, diseases were identified with specific collections of symptoms. In Alexander Tweedie's widely used work, *A system of practical medicine*, the word "disease" is defined as "a collection of disordered actions, called symptoms". Later we read that "morbid actions or phenomena may occur singly; but far more frequently they are observed in certain groups. The latter are what are generally known as special diseases, and are the subjects of nosology. The individual affections composing the groups are called *symptoms*, . . . which are themselves instances of disease. Thus the disease called phthisis is a collection of morbid states, such as emaciation, hectic fever, cough, expectoration, etc.; these are its symptoms: none of them individually could be called phthisis – a name which only belongs to them collectively."⁴ In this period writers were often remarkably indifferent to the *causes* of symptoms. In textbook treatments of many diseases, causes were simply not mentioned. If they were discussed, they were usually identified only in a common sense way or by the most casual observations. A whole range of divergent causes might be listed for a specific disease; conversely, specific causes were often associated with numerous disorders. One occasionally encounters the assertion that a specific disease can arise spontaneously. Causes were not generally used to explain symptoms or the course of the disease. Causes were not commonly used as a basis for classification: in 1849 the *Cyclopaedia of practical medicine* listed more than thirty different schemes for classifying diseases, not one of them was by cause.⁵

For example, in a lecture by M. Andral, delivered at the University of Paris in 1832 and reprinted in the *Lancet*, "hydrophobia" is said to denominate a "complete horror of fluids, reaching to such a degree, that their deglutition becomes almost impossible."⁶ Andral explicitly identifies hysterical hydrophobia as a genuine and even paradigmatic form of the disease. Immediately after the definition, Andral describes different "varieties of the disease". One variety is "a simple nervous perversion of no serious character . . . originating in a perturbed state of the functions of the nervous system . . . as is seen in hysteria [and] in many fevers." Other varieties of hydrophobia are identified as spontaneous or symptomatic. Symptomatic cases are those produced by the operation of a subtle contagion.

With the adoption of germ theory the situation changed radically. First, precise studies conclusively identified infestations of micro-organisms as a specific cause of sets of symptoms. Next, the presence or absence of a particular micro-organism or of its associated antibodies became the definitive criterion for the disease associated with that micro-organism. The *referents* of specific disease names were gradually changed from sets of symptoms to cases of infestation by the micro-organism. In this way the meanings of the names of infectious diseases were systematically changed. With these

⁴ J. A. Symonds, 'Pathological introduction', in Alexander Tweedie (editor), *A system of practical medicine*, London, Whittaker, 1840, vol. 1, pp. 2f.

⁵ John Conolly, 'Disease', in John Forbes, *et al.*, (editors), *The cyclopaedia of practical medicine*, Philadelphia, Lea & Blanchard, 1849, vol. 1, pp. 674-689, see pp. 688f.

⁶ M. Andral, 'Perversions of sensibility: hydrophobia', *Lancet*, 1832, i: 805-809, p. 806.

shifts it became possible to give coherent, unified explanations of the symptoms of a disease, the physical lesion, the course of the disease, its epidemiology, etc. Prior to germ theory such explanations would not have been possible; no single coherent explanation can account for the “horror of fluids” both of a person who has (what we now call) hydrophobia, and a person who has a hysterical reaction to dog bites. Contemporary physicians recognized these advantages of germ theory. In 1884 for example, Adolf Strümpell wrote, “One can justly claim that the scientific treatment of the etiology of diseases constitutes the most characteristic thrust of modern pathology, and . . . the secure establishment of the doctrine of organized, externally invading disease agents is until now the most beautiful and important achievement of this effort.”⁷ It was natural that the basic strategy of germ theory, which had proved so successful in dealing with the infectious diseases, would be emulated in other areas.

II

In the late decades of the nineteenth century, hysteria was among the most widely discussed diseases. Partly because Freud's early work focused on hysteria, the nineteenth-century discussion of that disorder has been the subject of continuing interest. Unfortunately, certain misconceptions, partially initiated by Freud himself, have been perpetuated in contemporary accounts. Because of these misconceptions, Freud's own contribution to the discussion has not been properly understood. It will be necessary to review standard medical opinions about hysteria during the 1880s, the time in which Freud was beginning his work.

In this period hysteria was consistently regarded as a functional nervous disorder where “functional” meant a disturbance of function without observable modifications in the affected organ. It was generally classified as a psychosis or as a neurosis. Martin Cohn (1883) classified hysteria as a functional psychosis, i.e. a disease “such that, given the current state of knowledge, no organic alteration of the central organs can be exhibited.”⁸ Cohn noted that Emanuel Mendel, Ausserordentlich Professor at Berlin, distinguished hysteria from other psychoses as a neurotic condition from which psychotic states follow.⁹ In 1884 Cohn and Mendel were reviewed in a comprehensive survey by a Dr. Schäfer of Berlin.¹⁰ Schäfer adopted the same definition. The first edition of Adolf von Strümpell's highly influential text, *Krankheiten des Nervensystems*, appeared in the same year; Strümpell defined hysteria as a functional disturbance without gross changes in the anatomy of the nervous system.¹¹ Also in

⁷ Adolf Strümpell, ‘Ueber die Ursachen der Erkrankungen des Nervensystems’, *Dt. Arch. klin. Med.*, 1884, 35: 1-17, p. 2.

⁸ Martin Cohn, ‘Ueber die Psychosen im kindlichen Alter’, *Arch. Kinderheilk.*, 1883, 4: 28-64, 101-107, p. 44. Joseph, ‘Ueber männliche Hysterie’, *Allg. med. ZentZtg.*, 1885, 54: cols. 631-634, 648f, 666f, 679-681, 698f, col. 667, says the same thing.

⁹ Mendel commonly referred to hysteria as a functional disease of the nervous system, e.g. ‘Ueber Hysterie beim männlichen Geschlecht’, *Dt. med. Wschr.*, 1884, 10: 241-244, p. 241. There is a report of this paper and the accompanying discussion in *Berl. klin. Wschr.*, 1884, 21: 314-317, 330f, 347f. An abridged version was also reprinted in Vienna: *Med.-chir. Zbl.*, 1885, 20: 271f, 283f, 295f.

¹⁰ [-] Schäfer, ‘Ueber Hysterie bei Kindern’, *Arch. Kinderheilk.*, 1884, 5: 401-428. Schäfer wrote his article with the help of Adolf Baginsky of Berlin. Freud mentions Baginsky as one of two Berlin physicians who allowed him to examine their neurological patients when he visited Berlin on his way from Paris to Vienna in 1886. Sigmund Freud, *The standard edition of the complete psychological works of Sigmund Freud*, James Strachey (editor), London, Hogarth Press, 1962, vol. 1, p. 14.

¹¹ Adolf Strümpell, *Krankheiten des Nervensystems*, Leipzig, F. C. W. Vogel, 1884, vol. 2, p. 417.

1884, J. Weiss, Dozent for psychiatry in Vienna, wrote that hysteria has symptoms and a course of development that could only belong to a psychosis, and that it must be treated by psychiatric methods only.¹² He wrote that the disease is entirely functional and that attributing the disease “to a palpable disorder of the central nervous system is entirely unthinkable.” Schäfer’s survey, together with the independent article by Weiss, provided the basis for an essay by Maximilian Herz (1885), Dozent for childhood diseases in Vienna.¹³ Herz adopted the definition of “functional psychosis” from Schäfer and Cohn and agreed that hysteria should be so classified. Herz added that numerous attempts to trace hysteria to anatomical lesions – Herz mentioned Theodor Meynert, Hermann Nothnagel, and others – had produced no significant results.¹⁴ By 1886 even pathological anatomists were identifying hysteria as a functional disorder.¹⁵ In this respect there were no significant differences between the Viennese and Jean Martin Charcot in Paris. Charcot observed that there are “a great number of morbid states, evidently having their seat in the nervous system, which leave in the dead body no material trace that can be discovered. Epilepsy, hysteria, even the most inveterate cases, [and] chorea . . . deny the most penetrating anatomical investigations.”¹⁶ Charcot identified such disorders as neuroses – a term defined by his English translator as “diseases of the nervous system apparently due to functional or dynamic causes; which are not, so far as we know, attended by any organic lesion.”¹⁷ This, of course, is virtually the same definition that appears so frequently in the German and Austrian literature of the period.

Hysteria was generally regarded as highly irregular; its irregularity was manifested in two respects: symptoms could change radically in a given patient, and symptoms could vary radically from patient to patient. Mendel cited Sydenham as having referred

¹² J. Weiss, ‘Die infantile Hysterie’, *Arch. Kinderheilk.*, 1884, 5: 451–461, pp. 457f. Weiss also cites Karl Moeli. Karl von Liebermeister says the same thing. ‘Ueber Hysterie und deren Behandlung’, *Samml. klin. Vortr.*, Innermedizin, 82: 2139–2158, p. 2147, reviewed in *Wien. med. Wschr.*, 1884, 34: 478.

¹³ Maximilian Herz, ‘Ueber Hysterie bei Kindern’, *ibid.*, 1885, 43: cols. 1305–1308, 1338–1342, 1368–1371, 1401–1405.

¹⁴ *Ibid.*, col. 1371. Kenneth Levin claims that “Theodor Meynert and his associates . . . dominated not only Viennese psychiatry but German psychiatry generally.” ‘Freud’s paper ‘On male hysteria’ and the conflict between anatomical and physiological models’, *Bull. Hist. Med.*, 1974, 48: 377–397, p. 378. This claim, which is no doubt based on Freud’s comments about Meynert, cannot be justified by the primary sources. In his very extensive review of the literature, Schäfer mentions over fifty authors from Germany, Austria, France, Denmark, Sweden, England, and America; Meynert is not among them. Cohn mentions Meynert but only in connexion with studies on the earliest reflex behaviour of newborn infants. In the discussion of hysteria in the second edition of Strümpell’s *Krankheiten des Nervensystems*, published in 1885, Charcot is mentioned ten times (all favourable), Meynert is not mentioned at all. Meynert is not mentioned by Weiss, who was Meynert’s colleague at the University of Vienna. Herz, who was also at the University of Vienna, says only that Meynert’s work on hysteria has produced no significant results. Of about two hundred articles on hysteria produced in Germany and Austria between 1880 and 1886 Charcot is mentioned about twenty times as frequently as Meynert. Levin’s only evidence that Meynert and his associates dominated psychiatry consists of quotations from books written in 1858, 1865, and 1870 in which Meynert is never mentioned (Meynert completed his training in 1870).

¹⁵ E.g., Robert Thomsen, ‘Ein Fall von tödlicher Neuropsychoses’, *Arch. Psychiat. Nervenkr.*, 1886, 17: 844–863, pp. 856–863.

¹⁶ J. M. Charcot, *Oeuvres complètes de J. M. Charcot*, M. M. Babinski, et al. (editors), Paris, Bureaux du Progrès Médical, 1890, vol. 3, pp. 14f.

¹⁷ J. M. Charcot, *Clinical lectures on diseases of the nervous system*, trans. by Thomas Savill, London, New Sydenham Society, 1889, vol. 3, p. 13.

to the disease as a a veritable Proteus displaying as many colours as the chameleon.¹⁸ Heinrich von Bamberger (1883) described hysteria as consisting of “disturbances in different parts of the body, often contradictory in nature, and highly variable, without any anatomical foundation being discovered in necropsy.”¹⁹ Weiss noted that one is justified in thinking of a hysterical condition whenever one encounters a group of symptoms that resembles some definite organic illness, but which departs in some respects from the nature or course of development of that organic disease. “There is hardly a symptom, whether of not we are in a position to ascribe it to a particular anatomical foundation, which cannot, either alone or with other symptoms, belong to the picture of hysteria.”²⁰ But in spite of these irregularities some physicians, most notably Charcot, felt they could detect certain patterns. Charcot himself characterized hysteria by the use of five “stigmata” which he felt were always present in a greater or lesser degree; these included (1) sensorial hemianaesthesia, “that stigma which almost surely characterizes the hysterical condition”; (2) the ovarian phenomenon, i.e. the phenomenon that in many women hysterics an attack could be provoked or arrested by direct pressure on an ovary; (3) the existence of hysteriogenic points which function similarly to the ovary in provoking and arresting attacks but whose location varies from one hysteric to another; (4) the manifestation of a definite series of stages in hysteric attacks; and (5) paraplegic or hemiplegic paralysis.²¹ This scheme was first clearly articulated in 1883,²² and the first German translation (Freud's) was published in 1886. Charcot's work was known and followed in Austria and Germany. Moriz Rosenthal (1882), who was in personal contact with Charcot, described hysteria in terms roughly compatible with Charcot's stigmata, and distinguished three major classes of hysterics depending on which symptoms were most pronounced.²³ Eduard Henoeh's text on childhood diseases, which went through eleven editions beginning in 1881, also contained a discussion of hysteria that was based on Charcot. Henoeh, like Rosenthal, distinguished classes of hysterics depending on which symptoms were most apparent: psychotic hallucinations, convulsions, motor disturbances including paralysis, and sensory disturbances including hemianaesthesia.²⁴ Henoeh's system was adopted by both Herz and Cohn.²⁵ Strümpell's discussion of hysteria in his text on the diseases of the nervous system was most heavily dependent on Charcot. In the second edition of his text, which was published in 1885, Strümpell cited Charcot ten times in his twenty-two-page discussion.²⁶ He discussed and adopted Charcot's five stigmata

¹⁸ Mendel, op. cit., note 9 above, p. 241.

¹⁹ Heinrich von Bamberger, 'Hysterie', *Allg. wien. med. Ztg.*, 1883, 28: 529f. According to Levin, Bamberger never wrote on hysteria. Levin, op. cit., note 14 above, p. 395.

²⁰ Weiss, op. cit., note 12 above, p. 452.

²¹ Charcot, op. cit., note 16 above, vol. 3, pp. 115f. Cf. Freud, op. cit., note 10 above, vol. 1, p. 11.

²² M. Charcot, 'Deux cas de contracture hystérique d'origine traumatique', *Progr. méd., Paris*, 1883, 11: 37-39, p. 39.

²³ Moriz Rosenthal, 'Untersuchungen und Beobachtungen über Hysterie', *Wien. med. Pr.*, 1879, 20: 569-572, 604-607, 633-636, 670-672, 737-741, 801-805. Rosenthal quotes a personal letter from Charcot (1878): "I am entirely of your opinion regarding the cerebral location of hysterical hemianaesthesia", col. 671.

²⁴ Eduard Heinrich Henoeh, *Vorlesungen über Kinderkrankheiten*, Berlin, August Hirschwald, 1881. Henoeh's chapter on hysteria was reprinted as 'Die hysterischen Affektionen der Kinder', in *Wien. med. Pr.*, 1881, 22: 916-918, 951f, 980-982, 1006-1009.

²⁵ Herz, op. cit., note 13 above, col. 1305; Cohn, op. cit., note 8 above, p. 52.

²⁶ Adolf Strümpell, *Krankheiten des Nervensystems*, 2nd ed., Leipzig, Vogel, 1885, vol. 2, pp. 450-471.

including the specific stages that Charcot identified as characteristic of hysteric attacks. Strümpell's discussion reflects a thorough grasp of all Charcot's main doctrines; since the text was among the most widely used texts in the field, it would certainly have been known in Vienna.

Standard attempts to define hysteria were symptomatic. Notice the following phrases: "hysteria, like neurasthenia, is only a symptom or a complex of symptoms . . ." (Herz); "if we seek the constituent elements of hysteria, the hysterical symptoms, . . ." (Tuczek); and "hysteria designates a series of the most variable symptom-complexes . . ." (Cohn).²⁷ Each of these suggests that hysteria was identified with certain combinations of symptoms. This identification seems particularly appropriate (indeed necessary) given that no organic lesions could be conclusively demonstrated in autopsies of hysterics – what could hysteria be besides the symptoms? Charcot's stigmata were obviously of this nature.²⁸ Weiss, Henoeh, Herz, Cohn, and Oppenheim all adopted symptomatic characterizations.²⁹ Ludwig Seeligmüller argued that chorea should be regarded as a form of hysteria since choreatics invariably display all the symptoms of hysteria.³⁰ F. Tuczek argued explicitly *against* attempting to define hysteria in any way other than symptomatically. His basic idea was that all other nervous diseases were defined in this way, and that hysteria should be so defined regardless of how the symptoms may come about.³¹ Given that other nervous disorders were also characterized symptomatically and that (as Charcot's own students admitted,³²) in some cases it was difficult or impossible to make differential diagnoses, the nervous disorders seemed to blend together. Physicians regularly suggested that the nervous disorders were ultimately all one, or that they differed only in degree.

Hysteria was generally regarded as caused by an ill-defined combination of

²⁷ Herz, op. cit., note 13 above, col. 1305. F. Tuczek, 'Zur Lehre von der Hysterie der Kinder', *Berl. klin. Wschr.*, 1886, 31: 511-515, 534-537, p. 511. Cohn, op. cit., note 8 above, p. 51.

²⁸ Charcot's commitment to a symptomatic definition for hysteria is illustrated in a dispute between himself and two German neurologists, Robert Thomsen and Herman Oppenheim. The Germans argued that a certain nervous syndrome, known as railroad spine, was different from classical hysteria; one (secondary) reason they advanced for differentiating the two disorders was that railroad spine and hysteria had different causal origins: the syndrome was always the result of a serious illness or a physical injury. Thomsen and Oppenheim, 'Ueber das Vorkommen und die Bedeutung der sensorischen Anästhesie bei Erkrankungen des centralen Nervensystems', *Arch. Psychiat. Nervenkr.*, 1884, 15: 559-583, 633-680, p. 666. In response Charcot totally ignored the causal factors which, from his point of view, were entirely beside the point. He observed that the symptoms displayed in the syndrome were exactly those of classical hysteria; for him this settled the issue. Charcot, op. cit., note 16 above, vol. 3, pp. 258f. Freud agreed with Charcot in this matter, but then, in an interesting theoretical manoeuvre, ended up using the traumatic cases of hysteria as a paradigm to explain the classical ones; he did this by assuming that the latter are always caused by suppressed emotional trauma. Freud, op. cit., note 10 above, vol. 3, p. 34.

²⁹ Weiss characterized hysteria by an absence of order among the symptoms, op. cit., note 12 above, p. 452. Henoeh rejected all theories of hysteria as untenable and claimed that we must be content with regarding a specific combination of neurotic symptoms as the expression of hysteria. Henoeh, op. cit., note 24 above, col. 916. Herz, op. cit., note 13 above, cols. 1305f, and Cohn, op. cit., note 8 above, pp. 51f, both follow Henoeh. Herman Oppenheim, 'Thatsächliches und Hypothetisches über das Wesen der Hysterie', *Berl. klin. Wschr.*, 1890, 27: 553-556, p. 554.

³⁰ Ludwig Seeligmüller, 'Ueber Chorea magna und ihre Behandlung', *Dt. med. Wschr.*, 1881, 7: 584.

³¹ Tuczek, op. cit., note 27 above, p. 511.

³² D. F. Ghilarducci, 'Contribution au diagnostic différentiel entre l'hystérie et les maladies organiques du cerveau', *Arch. Neurol.*, Paris, 1892, 24: 387-422; 1892, 25: 41-64, p. 388.

disposing and precipitating factors. In the 1850s cases of male hysterics were regularly being described in European medical literature; thirty years later, in the period we are considering, it was common knowledge in Vienna (and throughout Europe) that either sex was vulnerable.³³ In this period there was a great interest in child hysteria; this interest, together with the long recognition of male hysteria, completely exploded the old idea that hysteria was connected with movements or irritation of the uterus. Writers in the 1880s frequently began essays on hysteria by noting that this idea had been totally abandoned. Tuczek asserted that “associating hysteria with the uterus is like associating melancholie with black bile.”³⁴ Writers in our period identified a wide variety of possible causes of hysteria; these were classified as disposing or precipitating. Heredity and such factors as chronic illness, malnutrition, emotional instability, inferior ethnic origin, adverse climate or meteorological conditions, sexual abnormality, and persistent irritations (either physical or emotional) were mentioned as disposing factors. Even more precipitating factors were mentioned; these included (but were by no means limited to): sexual trauma, illness, infections of various kinds, emotional shocks, inadequate or excessive exercise, intellectual exertion, and fear. In this respect Charcot was entirely typical. Charcot distinguished predisposing and

³³ In 1849 John Conolly regarded the existence of male hysteria as uncontroversial. ‘Hysteria’, in Forbes, op. cit., note 5 above, vol. 2, pp. 570-572. In 1857 Moritz Heinrich Romberg discussed the question of male hysteria and concluded that it was entirely genuine. *Lehrbuch der Nervenkrankheiten*, 3rd ed., Berlin, A. Hirschwald, 1857, p. 563. In 1859 Paul Briquet published his *Traité clinique de thérapeutique de l’hystérie*, Paris, J. B. Bailliere, pp. 11-51, which contains statistical studies on the frequency of male hysteria. This work was very positively reviewed in *Jb. ges. Med.*, 1860, 1: 3:79. In 1868 Moriz Benedikt published a long article in the *Wien. med. Wschr.*, in which the existence of male hysteria is taken for granted: 1868, 18: 68-70, 81-83, 105-108, 121-124, col. 107. In 1880 Hermann Smidt published a fine discussion of male hysteria in which he cited dozens of cases reported in the literature of Germany, Austria, France, and England through the period from about 1840 through 1880. He quotes seventeenth- and eighteenth-century (and classical) writers who considered the existence of male hysteria an established fact. ‘Ueber das Vorkommen der Hysterie bei Kindern’, *Jb. Kinderheilk.*, 1880, 15: 1-22. In our period the following writers explicitly affirm the existence of male hysteria in their works cited above: Bamberger, Cohn, Hensch, Herz, Joseph, Mendel, Schäfer, Smidt, Seeligmüller, Thomsen, Tuczek, and Weiss.

Unsuspecting readers generally infer from Freud’s *Autobiographical study* that Freud introduced the concept of male hysteria into Vienna when he returned from studying with Charcot. Ilza Veith (*Hysteria: the history of a disease*, Chicago, University of Chicago Press, 1965, p. 263), and R. A. Cleghorn (‘Hysteria – multiple manifestations of semantic confusion’, *Canad. psychol. J.*, 1969, 14: 539-551, p. 540) both interpret Freud in this way. Hannah Decker writes that “the existence of male hysteria was not accepted generally in German medical circles.” *Freud in Germany*, New York, International Universities Press, 1977, p. 79. She notes that “between 1885 and 1902 only ten articles on male hysteria appeared in the German literature, all in a five-year period, 1895-1900” thus suggesting that the Germans were late in coming to recognize the existence of male hysteria. But her conclusion is based on a review of titles in the Surgeon-General’s *Index*; in fact virtually everyone in Germany and Austria who wrote on hysteria during the 1880s acknowledged its occurrence in males. Decker’s discussion refers to only two (of more than two hundred) journal articles on hysteria in the decade prior to 1890. Freud’s own comments seem intended to leave the impression that male hysteria was unknown in Vienna; in any case he regularly hints that physicians in Vienna both underestimated the frequency (or denied the existence) of male hysteria and supposed hysteria to depend on genital irritation. Freud, op. cit., note 10 above, vol. 1, pp. 11f, vol. 3, p. 21, vol. 20, p. 15. He may have derived this opinion from Charcot. Charcot, op. cit., note 16 above, vol. 3, p. 114. But in fact German and Austrian physicians seem to have regarded male hysteria as even more common than did Charcot. In Germany and Austria, Briquet’s estimate that one hysteric in twenty was male was regarded either as *reliable* (e.g. Mendel op. cit., note 9 above, p. 241; Schäfer, op. cit., note 10 above, p. 402; Joseph op. cit., note 8 above, col. 648) or possibly even as *too low* (*Med. Times, N. Y.*, 1884, 2: 195). On at least two occasions Charcot wrote that Briquet’s estimate was *too high*. Charcot, op. cit., note 16 above, vol. 3, pp. 89, 114.

³⁴ Tuczek, op. cit., note 27 above, p. 511.

provocative causes; the former included especially heredity, in a broad sense, as well as other factors; provocative causes included dog bites, lightning bolts, unrequited love, alcoholic and lead poisoning.³⁵ Charcot explicitly insisted that different cases of a single nervous disease, e.g. hysteria, could have a variety of different causes, and also that different diseases, e.g. hysteria and epilepsy, could have exactly the same cause.³⁶ Charcot also discussed cases of hysteria that “could be assigned to no cause.”³⁷ Charcot himself suggests that as a clinician he had little reason to concern himself with causes; his task was simply to portray the disease as he saw it.³⁸

As we can see, there were important similarities between the conception of hysteria in the 1880s and the conceptions of most diseases at the beginning of the century. Hysteria was defined and classified symptomatically; the etiological accounts were vague and inconsistent, the causes of hysteria were not used to explain other aspects of the disease. As the contrast between this confusion and the orderly scientific explanations of the infectious diseases became progressively more apparent, it was inevitable the neurologists and psychiatrists would look to germ theory as a model.

III

In 1884 Adolf Strümpell advocated a new approach to hysteria and to the other nervous disorders.³⁹ Strümpell observed that symptomatology and pathological anatomy could not advance the comprehension of any disease beyond a certain limited point. Even a complete microscopical description of a diseased organ could not satisfy the standards for comprehension that had been established for the infectious diseases by bacteriology. Such comprehension, Strümpell noted, could be achieved only when the symptoms and the anatomical lesions could themselves be explained as necessary developments from the original causes of the disease, and this required following the model of germ theory.

In 1888, P. J. Möbius offered an etiological characterization of hysteria and attempted to give causal explanations for its symptoms. “All those diseased modifications of the body are hysterical which are caused by ideas.”⁴⁰ Möbius admitted that he was not able to trace all hysterical symptoms to ideas; the subject himself may not be able to give an account of his internal processes. But it is a common experience that hysterical symptoms often come and go because of ideas. In what he calls an argument by analogy, Möbius alludes to Charcot’s findings that all hysterical

³⁵ J. M. Charcot, *Poliklinische Vorträge*, Vienna, Franz Deuticke, 1894, 1895, vol. 2, p. 31. Under the heading of heredity Charcot includes such factors as a father abandoning his family (*ibid.*, p. 6), alcoholic aunts (p. 227), and a suicidal father (p. 319), in addition to hysteric, epileptic, or nervous relatives. Even Freud objects when Charcot “makes an arthritic tendency in relatives figure as a hereditary neuropathic disposition.” (*ibid.*, vol. 1, p. 237). For the provocative causes see vol. 2, pp. 223, 393, 85, and 32 respectively.

³⁶ E.g., *ibid.*, pp. 32f, and vol. 1, pp. 371f.

³⁷ Charcot, *op. cit.*, note 16 above, vol. 1, p. 366.

³⁸ Charcot, *op. cit.*, note 35 above, vol. 2, p. 360. Cf. Freud, *op. cit.*, note 10 above, vol. 3, pp. 12f.

³⁹ Strümpell, *op. cit.*, note 7 above.

⁴⁰ P. J. Möbius, ‘Ueber den Begriff der Hysterie’, *Zbl. Nervenheilk.*, 1888, 11: 66-71. In 1893 Pierre Janet published an extensive review of attempts to characterize hysteria. These were classified under the headings: maladies of representation, doubling of personality, restriction of consciousness, and mental maladies. Janet considers about two dozen attempts and of these only Möbius’ is clearly and consistently etiological. Janet’s own account is also symptomatic. Janet does not call attention to the fact that Möbius is unique in this respect. Janet, ‘Quelques définitions récentes de l’hystérie’, *Arch. Neurol., Paris*, 1893, 25: 417-438; 26: 1-29.

Germ theory, hysteria, and Freud's early work in psychopathology

symptoms can be induced by hypnotic suggestion, and concludes that all hysterical symptoms are caused by ideas. Möbius observes that this definition is confirmed by clinical experience, but he also mentions the definition's theoretical and practical advantages: it yields conceptual clarity and unity by realigning the boundaries between hysteria and the other nervous disorders, it also provides a conceptual basis for existing psychiatric therapies and suggests new therapies as well.

Möbius' definition was explicitly intended to bring unity and coherence into the discussion of hysteria by using the same basic strategy that was employed in defining the infectious diseases.⁴¹ Möbius' essay was mildly influential: the definition was given serious critical attention in European medical literature, some writers adopted the definition, and, in their joint publication on hysteria, Freud and Josef Breuer gave more critical attention to Möbius than to anyone else.⁴² In 1894 several of Möbius' essays were reprinted in a volume entitled *Neurologische Beiträge*. Freud wrote to Wilhelm Fliess that Möbius' essays were "very well done; they are important on the subject of hysteria. His mind is the best among the neurologists; fortunately he is not on the track of sexuality."⁴³

In 1892 Strümpell delivered a lecture entitled 'On the origin and healing of diseases through ideas'.⁴⁴ The lecture carried one step further the project of explaining the nervous diseases by appealing to their causes. After prefatory comments Strümpell notes that the most characteristic thrust of contemporary medicine is the emphasis on the quest for causes of disease. Universal vacuous causes only superficially satisfy the need for causes; this need can be satisfied only through the discovery of causes that operate in every single case, only through a knowledge of their nature, the manner of their operation, the site of their influence, and the necessity of their consequence. Everyone knows, he continues, how much our opinions have been enriched and deepened in these respects in the last twenty years, particularly through work in the area of the infectious diseases. Strümpell then considers the influence of psychiatric techniques in the generation and healing of disease. Also in this area the quest for insight into causes has achieved a level from which the physician, freed from earlier prejudices, can obtain a clear and realistic perception of the actual situation. Strümpell considers some of the specific neuroses and shows how regarding them as essentially diseases of ideas could explain them and the therapeutic measures employed in their treatment. Between 1884 and 1892 both Möbius and Strümpell took steps toward an etiological account of hysteria; however, neither had the persistence and imagination to generate a theory with lasting impact. By the following year, 1893, Freud was beginning to develop just such a theory.

⁴¹ This becomes most clear in a subsequent publication, P. J. Möbius, 'Ueber die Eintheilung der Krankheiten', *Zbl. Nervenheilk.*, 1892, 15: 289-301. Möbius provides interesting arguments that pathological anatomy cannot provide an adequate basis for understanding or for classifying diseases, that the nervous disorders must be treated along exactly the lines illustrated by contemporary work in infectious diseases, and that this approach, which he regards as essentially new, will totally alter the conception of medicine.

⁴² Freud, op. cit., note 10 above, vol. 2, pp. 8n, 186-191, 215, 243, 248n.

⁴³ Sigmund Freud, *The origins of psycho-analysis: letters to Wilhelm Fliess*, Marie Bonaparte, et al., (editors), New York, Basic Books, 1954, p. 101.

⁴⁴ Adolf Strümpell, 'Ueber die Entstehung und die Heilung von Krankheiten durch Vorstellungen', *Berl. klin. Wschr.*, 1893, 30: 22-25.

IV

Freud's earliest medical studies emphasized neurology and anatomy. We know that Josef Breuer called Freud's attention to the remarkable case of Anna O. before Freud went to Paris in the autumn of 1885. Freud reported this case to Charcot, but, he wrote, "the great man showed no interest in my first outline of the subject, so that I never returned to it and allowed it to pass from my mind."⁴⁵ Apparently Freud began to study hysteria seriously when he was unable to obtain adequate laboratory facilities for the neurological studies that had been his first interest.⁴⁶ James Strachey estimates that this momentous shift in Freud's studies occurred in early December 1885.⁴⁷

After Freud returned to Vienna he presented a paper on male hysteria to the Viennese *Gesellschaft der Aerzte*.⁴⁸ In the paper, Freud discussed "what was completely novel" in the studies of Charcot: he claimed that prior to Charcot, hysteria had not been well defined, that no definitive symptomatology had been assigned to that disease. Freud objected to the "widespread prejudices" that hysteria was attributable to genital irritation, and he credited Charcot with having refuted this prejudice by demonstrating the unsuspected frequency of male hysterics. He further attributed to Charcot the discovery of special somatic signs by which the certain diagnosis of hysteria was made possible. "Thus," Freud concluded, by Charcot's efforts "hysteria was lifted out of the chaos of the neuroses, was differentiated from other conditions with a similar appearance, and was provided with a symptomatology which, . . . makes it impossible any longer to doubt the rule of law and order." These claims were certainly not impressive to Freud's audience: Charcot's attempts to systematize the symptomatology of hysteria were neither unknown nor unique. The "widespread prejudices" to which Freud objected had, in fact, been abandoned years earlier, and Briquet's estimation of the frequency of male hysteria, which formed the basis of Charcot's opinions, had been accepted by the Viennese and Germans for years.⁴⁹ Freud, who first became seriously interested in hysteria while in Paris, may simply not have been familiar with existing literature on the disease. In any case, Freud's misconceptions, together with his unrestrained admiration and loyalty for Charcot, were no doubt responsible for the disappointing reception his paper received.⁵⁰

⁴⁵ Freud, op. cit., note 10 above, vol. 20, pp. 19f.

⁴⁶ Ibid., vol. 1, pp. 8f.

⁴⁷ Ibid., p. 4.

⁴⁸ Freud's paper has not survived. The nature of his comments and the events of the meeting are known only through reports published in Viennese and German medical journals (for a list see Ellenberger, op. cit., note 3 above, p. 554) and through a preliminary report that Freud presented to the Medical Faculty in Vienna. Freud, op. cit., note 10 above, vol. 1, pp. 5-15.

⁴⁹ See note 33 above.

⁵⁰ Freud wrote: "The high authorities had rejected my innovations." Freud, op. cit., note 10 above, vol. 20, p. 15. But there were no innovations. Freud also wrote that Bamberger rejected his paper as "incredible" and the context suggests that the remark was in reference to the existence of male hysteria. But Bamberger had himself published a paper in which male hysteria was recognized. Levin, who feels that this view of Freud's reception "reduces Freud to little more than a fool," sees things quite differently. Levin, op. cit., note 14 above, p. 378. According to Levin, Freud was poorly received because he was introducing the "new and revolutionary" concept that hysteria was a functional illness rather than a disorder "due to an anatomical lesion of the brain." Levin writes that "patho-anatomical models [for hysteria] totally prevailed in Vienna in the years preceding 1886" (p. 396). Levin holds that "it was Freud's trip to Paris which marked the first step in the evolution of [the functional concept] . . . the most fruitful of the new pathopsychologies generated in the last two decades of the nineteenth century" (p. 397). But this contention is entirely wrong. As we have

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In the years between 1886 and 1893 there was little published on which to base an opinion of Freud's thought about hysteria. Apart from an unsigned article on hysteria in Villaret's medical encyclopaedia, which Freud almost certainly wrote,⁵¹ there are only items of minor interest. If the article is Freud's it shows that as late as 1888 Freud had not departed significantly from the position taken in his report: hysteria is still treated as an orderly disease accurately characterized by Charcot's stigmata; there is no indication that Freud had become interested in an etiological characterization of hysteria; the causes of the disease are still the familiar disposing and precipitating factors. Perhaps the most significant difference between the report and the 1888 article is that the article suggests a familiarity with existing literature on hysteria.⁵²

Freud's publications in 1893 show a radical departure from Charcot's symptomatic characterization of hysteria. In Freud's translation of Charcot's lectures published in that year, we find a series of footnotes very clearly indicating the new direction of Freud's thought. In one footnote, Freud objects that Charcot's etiology did not separate the disposition to neuroses from the disposition to organic nervous disorders.⁵³ Given a purely symptomatic conception of the neuroses there would be no reason to expect etiology to provide for this separation. Indeed, such a provision would be impossible because, as everyone knew, symptomatically defined functional disorders could be caused by the same physical factors that, on other occasions, caused related organic disorders.⁵⁴ Various contemporary writers, after adopting a symptomatic definition for hysteria, explicitly denied that hysteria could be causally distinguished from other organic or functional disorders. One would expect etiology to make this separation only if one believed that different diseases necessarily have different causes. In fact, in an 1892 publication, Möbius rejected as nonsense (*unsinning*) the idea that different diseases could have the same cause.⁵⁵ At the time he was translating Charcot's lectures, Freud knew this publication and, in a footnote, he recommended it to Charcot's readers.⁵⁶ In the same publication Möbius observed (as

seen, the functional concept of hysteria had become standard in Vienna – as everywhere else in Europe – for years before Freud even went to Paris. It is true that Rosenthal and Meynert continued to look for organic lesions; so did Luys in France. Such persons were in a rapidly declining minority and, by 1886, no longer exerted an important influence on thought about hysteria. Rather than admitting that Freud ascribed to the Viennese opinions that they had abandoned years earlier, Levin follows Freud and mistakenly ascribes those same opinions to Freud's Viennese contemporaries. Decker makes a similar mistake (op. cit., note 33 above, pp. 80ff.).

⁵¹ For a review of the evidence see Paul Vogel, 'Ein erste, unbekannt gebliebene Darstellung der Hysterie von Sigmund Freud', *Psyche, Berl.*, 1953, 7: 481-485.

⁵² For example, there is an argument not just that hysteria must be treated as functional since no lesions can be discovered, but that no lesions can exist since the symptoms are incompatible with what is known about the nervous system. Freud, op. cit., note 10 above, vol. 1, p. 49. This is an argument that Charcot never used and that appears only in Weiss, op. cit., note 12 above, p. 458. There are also other passages in the article that are similar in content to ideas that had previously been uniquely associated with Mendel and with Liebermeister, although in none of these cases is it possible to establish a positive influence. In certain formal respects, the article resembles the discussion of hysteria in Strümpell, op. cit., note 26 above.

⁵³ Charcot, op. cit., note 35 above, vol. 1, p. 237.

⁵⁴ For example, M. A. Souques was one of a whole series of authors (including Charcot) who exhibited a lead worker displaying hysterical symptoms of lead poisoning and who insisted that those symptoms were caused by exposure to lead. 'Note sur un cas d'hémiplégie hystérique chez un saturnin', *Gaz. méd. Paris*, 1889, 6: 17-19.

⁵⁵ Möbius, op. cit., note 41 above, p. 290.

⁵⁶ Charcot, op. cit., note 35 above, vol. 1, p. 149. This footnote is not included in the standard edition of Freud's works.

Strümpell had before) that redefining the nervous diseases in causal terms would entail reclassifying them. Several of Freud's footnotes express objections to Charcot's scheme for classifying the nervous disorders, the so-called *famille neuropathique*. In one footnote Freud explains that his objections to Charcot's scheme were at least partially the result of his work on the etiology of tabes.⁵⁷ In another footnote Freud observes that his theory of "hysterical counterwill" connects together various hysterical symptoms and thereby throws light on the mechanism of the hysterical condition.⁵⁸ At about the same time, in a preliminary draft for their subsequent joint publication, Freud and Josef Breuer object that Charcot had only described hysteria and that "this description throws no light at all on any connection there may be between the different phases, on the significance of attacks in the general picture of hysteria, or on the way in which attacks are modified in individual patients."⁵⁹ Strümpell and Möbius had insisted it was precisely the etiological definitions of germ theory that had thrown light on just these factors in the case of the infectious diseases.

Thus there are numerous indications in Freud's writings from 1893 and 1894 that he was moving away from Charcot's symptomatic treatment of hysteria and that he was attracted by an etiological approach. It has been universally recognized that Freud began to criticize Charcot in 1893, but the significance of that criticism has been generally overlooked. For example, Jones writes "What Freud maintained as the result of his observations was that, whenever a thorough investigation of the patient could be carried out, sexual etiological factors would be found which were different in [hysteria and the anxiety neuroses], this was his justification for separating them."⁶⁰ There is no indication that Jones sees any novelty or particular importance in this new strategy. Yet these steps, which had been advocated by Möbius and Strümpell, fundamentally severed Freud's work from the ideas of other predecessors; he adopted an orientation, never to be abandoned, that brought his work on psychopathology into harmony with the prevailing orientation of medical research in his time. These facts can be ignored only at the price of failing to see one truly revolutionary aspect in Freud's approach.

We must now consider Freud's work in the few years following 1893. It is not necessary to trace the evolution of Freud's thought or even to summarize his ultimate views. Our object will be only to exhibit Freud's quest for etiological characterizations of the nervous disorders, especially hysteria, and his use of those characterizations to provide explanations that were exactly analogous to the explanations that were, at the same time, being based on the etiological definitions of the infectious diseases. To accomplish this it will be necessary only to review certain prominent themes in Freud's writings through 1896, the year in which Freud published both 'Heredity and the etiology of the neuroses' and 'The etiology of hysteria'.

⁵⁷ Charcot, *op. cit.*, note 35 above, vol. 1, p. 8, cf. pp. 4, 404, 417. Cf. Freud, *op. cit.*, note 10 above, vol. 3, p. 23.

⁵⁸ Charcot, *op. cit.*, note 35 above, vol. 1, p. 137.

⁵⁹ Freud, *op. cit.*, note 10 above, vol. 1, p. 151.

⁶⁰ Ernest Jones, *The life and work of Sigmund Freud*, New York, Basic Books, 1953, vol. 1, p. 256. Jones mentions Möbius only as one of "a few workers in Germany . . . from whom . . . Freud could have derived but very little" (p. 370). Henri Ellenberger and Ola Andersson also completely miss this change in Freud's orientation. Andersson writes "In the late 1880s and 1890s P. J. Möbius and A. v. Strümpell had published papers on the traumatic neuroses and on hysteria in which they espoused views very similar to those of Charcot." *Studies in the prehistory of psychoanalysis*, Stockholm, Svenska Bokförlaget, 1962, p. 115.

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In 'Heredity and the etiology of the neuroses' Freud asks: "Is it possible to establish a constant etiological relation between a particular cause and a particular neurotic effect, in such a way that each of the major neuroses can be attributed to a specific etiology?" The answer is that each neurosis "has as its immediate cause one particular disturbance of the economics of the nervous system" and in particular, disturbances of "the subject's sexual life, whether they lie in a disorder of his contemporary sexual life or in important events in his past life."⁶¹ After considering the specific causes of some of the other neuroses he writes: "A passive sexual experience before puberty: this, then, is the specific etiology of hysteria."⁶² In 'Further remarks on the neuro-psychoses of defence' we read: "In order to cause hysteria, it is not enough that there should occur . . . an event which touches [the subject's] sexual existence and becomes pathogenic through the release and suppression of a distressing affect. On the contrary, these sexual traumas must have occurred in early childhood (before puberty), and their content must consist of an actual irritation of the genitals."⁶³ Finally, in 'The etiology of hysteria' we find this passage: "I therefore put forward the thesis that at the bottom of every case of hysteria there are one or more occurrences of premature sexual experience, occurrences which belong to the earliest years of childhood but which can be reproduced through the work of psychoanalysis in spite of the intervening decades. I believe that this is an important finding, the discovery of a *caput Nili* in neuropathology."⁶⁴ How are such passages to be understood? Freud frequently suggests that these claims are simply empirical discoveries from clinical observation.⁶⁵ Indeed, it is possible that the theses originated in just that way. However, their logical role in Freud's thought is *not* simply that of empirical generalizations.

Freud started out believing that Charcot's symptomatic characterization of hysteria was relatively precise. Perhaps for this reason Freud was careful to present his etiological account as a discovery based on Charcot's symptomatic definition. However, there are indications that at an early stage Freud himself regarded the etiological discovery as more fundamental than a simple empirical generalization. In his *Autobiographical study*, for example, Freud explains that Breuer's discoveries in the treatment of Anna O. "seemed to me to be of so fundamental a nature that I could not believe it could fail to be present in any case of hysteria if it had been proved to occur in a single one."⁶⁶ In letters to Fliess written in 1892 and 1893 – the first years in which there is any evidence that he was departing from Charcot's symptomatic characterization and only four years after the entirely orthodox article in the encyclopaedia – Freud insists "no neurasthenia or analogous neurosis can exist without a disturbance of the sexual function," and "the contention which I am putting forward and desire to test by observation is that neurasthenia is always *only* a sexual neurosis."⁶⁷

⁶¹ Freud, *op. cit.*, note 10 above, vol. 3, p. 149.

⁶² *Ibid.*, p. 152.

⁶³ *Ibid.*, p. 163. I emphasize: that Freud later modified or abandoned these theses in the light of his discovery of the Oedipus complex in no way affects my argument. Subsequent developments reinforce my claim that Freud's characterization of the nervous disorders was etiological.

⁶⁴ *Ibid.*, p. 203.

⁶⁵ E.g., *ibid.*, pp. 52, 99.

⁶⁶ *Ibid.*, vol. 20, p. 21.

⁶⁷ Freud, *op. cit.*, note 43 above, pp. 65f.

Only three years later, the sexual etiology of hysteria had become *definitional*: in 'Heredity and the etiology of the neuroses', Freud sets forth a "nosographic innovation" which is the result of the researches into the etiology of the major neuroses.⁶⁸ His innovation is a fourfold scheme in which each specific neurosis is attributed to a particular disorder in the subject's sexual behaviour. "What gives its distinctive character to my line of approach," Freud wrote, "is that I elevate these sexual influences to the rank of specific causes, that I recognize their action in every case of neurosis, and finally that I trace a regular parallelism, a proof of a special etiological relation between the nature of the sexual influence and the pathological species of the neurosis."⁶⁹ In the next few pages Freud discusses neurasthenia, other anxiety neuroses, hysteria, and obsessional neuroses; each of these is differentiated from the others by the specific pathological etiology which causes the symptoms. On the other hand, in 'The neuro-psychoses of defence' Freud identifies two "extreme forms of hysteria" which do not conform to a characterization of hysteria given by Pierre Janet. Both forms are defined etiologically.⁷⁰ Similarly, in a long paper on anxiety neuroses, Freud distinguishes six forms of neuroses found in women, four found in men, and two found in both sexes; all 12 forms are defined etiologically.⁷¹ It is clear that Freud uses his etiological account of the nervous disorders to generate a nosology more coherent, rational, and precise than had been possible before.

However, as Freud saw, the etiological definitions and the nosological innovations were not an end in themselves. In an early draft of their book on hysteria, Freud and Breuer objected that Charcot's description explained virtually nothing about the disease.⁷² In his writings, by contrast, Freud uses the etiological account of the nervous disorders to explain an incredible variety of phenomena among which are the following: certain hysterical symptoms, the incidence of hysteria and the hysterogenic zones, the response of hysterics to hypnosis, certain similarities among the neuroses, patterns of incidence of anxiety neuroses among married couples, neurasthenia occurring in some cases of sexual abuse, the suppression of those events that cause specific cases of hysteria, the predominance of hysteria among women and of obsessional neurosis among men, the apparent familial neurotic disposition and various pathological symptoms, habits, and phobias, the course of development of obsessional neuroses, the success and failure of various therapeutic measures, the rare occurrence of hysteria in the lower social orders,⁷³ and much much more. Moreover, Freud considered observed facts that could *not* be explained as possible weaknesses in his theory.⁷⁴ Freud commented in a discussion of organic lesions that the physician's representation of the causes and alterations of these lesions must be right "for by it he is able to understand the details of the illness."⁷⁵ By 1896 Freud wrote that "the symptoms of hysteria can only be understood if they are traced back" to etiological factors.⁷⁶ In a lecture of the same year he asserted, "In the sole attempt to explain the

⁶⁸ Freud, op. cit., note 10 above, vol. 3, p. 146.

⁶⁹ Ibid., p. 147.

⁷⁰ Ibid., pp. 46f.

⁷¹ Ibid., pp. 99-102.

⁷² Ibid., vol. 1, p. 151.

⁷³ Ibid., vol. 3, pp. 34, 153, 163, 50, 99, 101-110, 113, 111, 154, 156, 165, 209, 169, 130f, 146, 195, 211.

⁷⁴ Charcot, op. cit., note 35 above, vol. 1, p. 314.

⁷⁵ Freud, op. cit., note 10 above, vol. 11, pp. 11f. ⁷⁶ Ibid., vol. 3, p. 163.

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physiological and physical mechanism of hysteria which I have been able to make in order to correlate my observations, I have come to regard the participation of sexual motive forces as an indispensable premise."⁷⁷ Thus, at least by 1896, it was the explanatory force that Freud found compelling in the etiological account.

Because the theoretical advantages of his new approach were so great, Freud was willing to maintain his account even in the face of apparently incompatible clinical evidence. So far as Freud knew at this time, the case of Anna O. was an exception to his theory.⁷⁸ In a letter to Fliess in 1893 Freud admitted that it required some courage to insist on his etiological theories in the face of intractable clinical evidence, and, in another letter, he confessed that "the connection between obsessional neurosis and sexuality does not always lie so near the surface . . . if it had been sought for by anyone less obstinately wedded to the idea, it would have been overlooked."⁷⁹

We now see certain parallels between Freud's approach to psychopathology and work that was being done at about the same time in the pathology of infectious diseases. At least initially Freud and Breuer saw their work as closely associated with the positions of Möbius and Strümpell,⁸⁰ and they, in turn, saw their work as modelled on germ theory. Möbius and Strümpell explicitly set out to do for the nervous disorders what had been accomplished in the infectious diseases by using an etiological approach. It is possible that Freud was positively influenced by the strategy of germ theory in his own orientation toward the nervous diseases. Freud never explicitly identified this influence. However, he did use the contemporary infectious account of tuberculosis as an analogy in explaining and justifying some aspects of his own views about the causes of anxiety neuroses,⁸¹ and certain of the metaphors chosen by Freud and Breuer suggest also that they were aware of the connexion between their work and germ theory.⁸² In any event, Freud's work on psychopathology ended up exactly in harmony with the main orientation of the medical research of his time. Moreover, given that the successes of germ theory were so highly esteemed by Freud's contemporaries, the fact that Freud's theory exploited the same basic explanatory strategy must have made this etiological theory more appealing than other simultaneous accounts, such as Janet's, that were not etiological and could not offer the same explanatory force.

V

Why is it, then, that none of Freud's commentators has identified this parallel before? In part this may be a simple lack of historical perspective. In our time it seems so obvious that diseases should be characterized and classified by causes that Freud's use of that strategy appears to require no special discussion. But in Freud's time the situation was quite different: this approach was still relatively new even in the infectious diseases. Before 1884 no one seems even to have conceived of using such an

⁷⁷ *Ibid.*, p. 200. ⁷⁸ *Ibid.*, vol. 20, p. 26; there were other apparent exceptions, e.g., vol. 2, p. 14.

⁷⁹ Freud, *op. cit.*, note 43 above, pp. 78, 81.

⁸⁰ Freud, *op. cit.*, note 10 above, vol. 2, pp. 7f, 215; vol. 3, p. 51.

⁸¹ *Ibid.*, vol. 2, p. 187; vol. 3, p. 137, 209; and cf. vol. 3, p. 129.

⁸² For example the metaphor of the "foreign body", see *ibid.*, vol. 2, p. 6; vol. 3, pp. 35, 244, etc. Freud later referred to this metaphor as "physiological", *ibid.*, vol. 20, p. 23; one modern commentator sees this metaphor as a manifestation of Breuer's reluctance to abandon the nineteenth-century theory that "all disease entities trace back ultimately to some material cause." Philip Rieff, 'Introduction', in Sigmund Freud, *The history of the psychoanalytic movement*, New York, Collier Books, 1972, p. 14.

approach to nervous disorders; before 1888-1892 no one had really tried to do so; the minority who then tried such an approach were not consistent and were generally misunderstood. Freud was certainly the first to use this approach to provide anything resembling a coherent scientific theoretical explanation of the nervous diseases.

In recent years Freud's work has been eulogized as revolutionary – as the introduction of a new paradigm in science.⁸³ Freud's admirers (beginning with Freud himself) have compared him with Darwin and Copernicus.⁸⁴ Viewed in relation to any of Freud's recognized "sources", what he did was genuinely and literally revolutionary. Charcot – like every other late nineteenth-century physician who dealt with nervous disorders – started with symptoms and ended up with total nonsense in the discussion of causes. The result was that there were no coherent explanations of anything. Freud started with causes and explained the symptoms, as well as many other facets of nervous diseases, and, ultimately, everything from jokes and dreams to spelling errors. What could be more revolutionary than that? If we view Freud against the background of his recognized sources he achieved, almost singlehandedly, a revolution in thought. However, if we inquire into the nature of the revolution, and if we view Freud against the background of nineteenth-century medicine, then his work takes on the appearance of an ingenious application of a method that was already being employed with enormous success in other areas – a method that Freud elaborated but did not create. From this point of view, therefore, we must be more cautious about describing Freud as a paradigm initiator or scientific revolutionary. These facts, too, may relate to his commentators' inability to see what must be among the crucial factors that directed his work and helped to secure its acceptance.

It now appears that the model of comprehension first presupposed in germ theory underlies our way of thinking about whole classes of diseases that have nothing whatsoever to do with micro-organisms. The current pervasiveness of this way of thinking is illustrated by the fact that none of Freud's commentators sees any change at all when Freud completely reversed himself and adopted it. Thus, while grasping the relation between Freud's work and germ theory may call into question Freud's role as paradigm initiator, by so much the more does it assure in that role those who first articulated the conceptual approach that underlies germ theory.

SUMMARY

Nineteenth-century medicine was revolutionized by the adoption of germ theory. This involved much more than simply recognizing that various diseases were caused by micro-organisms. Adoption of germ theory entailed fundamental changes in the concept of disease, in nosology and diagnosis, and in standards of explanation in medical science. The results of this revolution were most striking in work on the infectious diseases, but this work was quickly emulated in other areas of medicine. A careful review of nineteenth-century medical literature shows that Freud was the first to apply successfully the theoretical strategy of germ theory to hysteria. By failing to recognize the extent to which Freud's early work was based on this existing strategy, his commentators have misunderstood the exact nature of his accomplishment and overestimated his originality.

⁸³ Md. Mujeeb-ur-Rahman (editor), *The Freudian paradigm: psychoanalysis and scientific thought*, Chicago, Nelson-Hall, 1977. And if, *per impossibile*, someone should miss the allusion, the first section of the book bears the unlikely heading 'Freud and the structure of scientific revolutions'.

⁸⁴ For a review and references see Nigel D. Walker, 'A new Copernicus?' and especially David Shakow and David Rapaport, 'Darwin and Freud: a comparison of receptions,' both reprinted in Mujeeb-ur-Rahman, *op. cit.*, note 83 above, pp. 35-42 and 43-63.