(*b*) English Translations:

Studies in Hysteria

1909 *S.P.H.*, 1–120. (1912, 2nd. ed.; 1920, 3rd. ed.; 1922, 4th. ed.) (Tr. A. A. Brill.) (In part only: omitting the case histories of Fräulein Anna O., Frau Emmy von N. and Katharina, as well as Breuer's theoretical chapter.)

1936 New York: Nervous and Mental Disease Publishing Co. (Monograph Series No. 61.) Pp. ix + 241. (Tr. A. A. Brill.) (Complete, except for omitting Freud's extra footnotes of 1925.)

The present, entirely new and complete translation by James and Alix Strachey includes Breuer's contributions, but is otherwise based on the German edition of 1925, containing Freud's extra footnotes. The omission of Breuer's contributions from the two German collected editions (*G.S.* and *G.W.*) led to some necessary changes and additional footnotes in them, where references had been made by Freud in the original edition to the omitted portions. In these collected editions, too, the numbering of the case histories was altered, owing to the absence of that of Anna O. All these changes are disregarded in the present translation.—Abstracts both of the 'Preliminary Communication' and of the main volume were included in Freud's early collection of abstracts of his own works (1897*b*, Nos. XXIV and XXXI).

(1)

Some Historical Notes on the Studies

The history of the writing of this book is known to us in some detail.

Breuer's treatment of Fräulein Anna O., on which the whole work was founded, took place between 1880 and 1882. By that time Josef Breuer (1842–1925) already had a high reputation in Vienna both as a physician with a large practice and as a man of scientific attainments, while Sigmund Freud (1856–1939) was only just qualifying as a doctor.[1] The two men had, however, already been friends for some years. The treatment ended early in June, 1882, and in the following November Breuer related the remarkable story to Freud, who (though at that time his main interests were centred on the anatomy of the nervous system) was greatly impressed by it. So much so, indeed, that when, some three years later, he was studying in Paris under Charcot, he reported the case to him. 'But 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.' (*An Autobiographical Study*, 1925*d*, Chapter II.)

[1] Much of the material in what follows is derived from Ernest Jones's life of Freud (Vol. I, and especially Chapter XI).

Freud's studies under Charcot had centred largely on hysteria, and when he was back in Vienna in 1886 and settled down to establish a practice in nervous diseases, hysteria provided a large proportion of his clientèle. To begin with he relied on such currently recommended methods of treatment as hydrotherapy, electro-therapy, massage and the Weir Mitchell rest-cure. But when these proved unsatisfactory his thoughts turned elsewhere. 'During the last few weeks', he writes to his friend Fliess on December 28, 1887, 'I have taken up hypnosis and have had all sorts of small but remarkable successes.' (Freud, 1950*a*, Letter 2.) And he has given us a detailed account of one successful treatment of this kind (1892–3*b*). But the case of Anna O. was still at the back of his mind, and 'from the first', he tells us (1925*d*) 'I made use of hypnosis in *another* manner, apart from hypnotic suggestion'. This 'other manner' was the cathartic method, which is the subject of the present volume.

The case of Frau Emmy von N. was the first one, as we learn from Freud (pp. 48 and 284), which he treated by the cathartic method.[1] In a footnote added to the book in 1925 he qualifies this and says it was the first case in which he made use of that method 'to a large extent' (p. 105); and it is true that at this early date he was still constantly employing hypnosis in the conventional manner—for giving direct therapeutic suggestions. At about this time, indeed, his interest in hypnotic suggestion was strong enough to lead him to translate one of Bernheim's books in 1888 and another in 1892, as well as to

[1] A remark on p. 103 almost seems to imply, on the other hand, that the case of Frau Cäcilie M. (mentioned below) preceded that of Frau Emmy. But this impression may perhaps be due to an ambiguity in the phrasing of the sentence.

pay a visit of some weeks to the clinics of Liébeault and Bernheim at Nancy in the summer of 1889. The extent to which he was using therapeutic suggestion in the case of Frau Emmy is shown very clearly by his day-to-day report of the first two or three weeks of the treatment, reproduced by him from 'the notes which I made each evening' (p. 48). We cannot unluckily be certain when he began this case (see Appendix A., p. 307); it was in May either of 1888 or of 1889—that is, either about four or about sixteen months after he had first 'taken up hypnotism'. The treatment ended a year later, in the summer of 1889 or 1890. In either alternative, there is a considerable gap before the date of the next case history (in chronological order, though not in order of presentation). This was the case of Fräulein Elisabeth von R., which began in the autumn of 1892 (p. 135) and which Freud describes (p. 139) as his 'first full-length analysis of a hysteria'. It was soon followed by that of Miss Lucy R., which began at the end of the same year (p. 106).[1] No date is assigned to the remaining case, that of Katharina (p. 125). But in the interval between 1889 and 1892 Freud certainly had experience with other cases. In particular there was that of Frau Cäcilie M., whom he 'got to know far more thoroughly than any of the other patients mentioned in these studies' (p. 69 *n.*) but whose case could not be reported in detail owing to 'personal considerations'. She is however frequently discussed by Freud, as well as by Breuer, in the course of the volume, and we learn (p. 178) from Freud that 'it was the study of this remarkable case, jointly with Breuer, that led directly to the publication of our "Preliminary Communication" '.[2]

[1] It is to be noted that neither of these last two analyses had been more than started at the time of the publication of the 'Preliminary Communication'.

[2] The question of when it was that Freud first began using the cathartic method is complicated still further by a statement made by him in 1916. The circumstances were these. At the International Medical Congress held in London in 1913, Pierre Janet had distinguished himself by making an absurdly ignorant and unfair attack on Freud and psycho-analysis. A reply was published by Ernest Jones in the *Journal of Abnormal Psychology*, 9 (1915), 400; and a German translation of this appeared in the *Int. Z. Psychoanal.*, 4 (1916), 34. In the course of his diatribe Janet had said that whatever was of the slightest value in psycho-analysis was entirely derived from his own early writings, and in traversing this assertion Jones had remarked that, though it was true that the actual publication of Breuer and Freud's findings was later than that of Janet's (which were published in 1889), the work on which their first paper was based preceded Janet's by several years. 'The co-operation of the two authors', he went on, 'antedated their first communication by as much as ten years, and it is expressly stated in the *Studien* that one of the cases there reported was treated by the cathartic method more than fourteen years before the date of the publication.' At this point in the German translation (ibid., 42) there is a footnote signed 'Freud', which runs as follows: 'I am obliged to correct Dr. Jones on a point which is inessential so far as his argument is concerned but which is of importance to me. All that he says on the priority and independence of what was later named psycho-analytic work remains accurate, but it applies only to *Breuer's* achievements. My own collaboration began only in 1891–2. What I took over I derived not from Janet but from Breuer, as has often been publicly affirmed.' The date given here by Freud is a puzzling one. 1891 is two or three years too late for the beginning of the case of Frau Emmy and a year too early for that of Fräulein Elisabeth.

The drafting of that epoch-making paper (which forms the first section of the present volume) had begun in June 1892. A letter to Fliess of June 28 (Freud, 1950*a*, Letter 9) announces that 'Breuer has agreed that the theory of abreaction and the other findings on hysteria which we have arrived at jointly shall also be brought out jointly in a detailed publication'. 'A part of it', he goes on, 'which I at first wanted to write alone, is finished.' This 'finished' part of the paper is evidently referred to again in a letter to Breuer written on the following day, June 29, 1892 (Freud, 1941*a*): 'The innocent satisfaction I felt when I handed you over those few pages of mine has given way to . . . uneasiness.' This letter goes on to give a very condensed summary of the proposed contents of the paper. Next we have a footnote added by Freud to his translation of a volume of Charcot's *Leçons du Mardi* (Freud, 1892–3*a*, 107), which gives, in three short paragraphs, a summary of the thesis of the 'Preliminary Communication' and refers to it as being 'begun'.[1] Besides this, two rather more elaborate drafts have survived.[2] The first (Freud, 1940*d*) of these (in Freud's handwriting, though stated to have been written jointly with

[1] It is not possible to date this precisely; for though Freud's preface to his translation is dated 'June 1892', the work came out in parts, some of which were published quite late in 1893. The footnote in question, however, appears on a relatively early page of the book, and may therefore be dated with fair certainty to the summer or autumn of 1892.

[2] All of these drafts and summaries will be found in full in the first volume of the *Standard Edition*.

Breuer) is dated 'End of November 1892'. It deals with hysterical attacks and its contents were mostly included, though in different words, in Section IV of the 'Preliminary Communication' (p. 13 ff.). One important paragraph, however, concerned with the 'principle of constancy', was unaccountably omitted, and in this volume the topic is treated only by Breuer, in the later part of the work (p. 197 ff.). Lastly there is a memorandum (Freud, 1941*b*) bearing the title 'III'. This is undated. It discusses 'hypnoid states' and hysterical dissociation, and is closely related to Section III of the published paper (p. 11 ff.).

On December 18, 1892 Freud wrote to Fliess (1950*a*, Letter 11): 'I am delighted to be able to tell you that our theory of hysteria (reminiscence, abreaction, etc.) is going to appear in the *Neurologisches Centralblatt* on January 1, 1893, in the form of a detailed preliminary communication. It has meant a long battle with my partner.' The paper, bearing the date 'December 1892', was actually published in two issues of the periodical: the first two Sections on January 1 and the remaining three on January 15. The *Neurologisches Centralblatt* (which appeared fortnightly) was published in Berlin; and the 'Preliminary Communication' was almost immediately reprinted in full in Vienna in the *Wiener medizinische Blätter* (on January 19 and 26). On January 11, while the paper was only half published, Freud gave a lecture on its subject-matter at the Wiener medizinischer Club. A full shorthand report of the lecture, 'revised by the lecturer', appeared in the *Wiener medizinische Presse* on January 22 and 29 (**34**, 122–6 and 165–7). The lecture (Freud, 1893*h*) covered approximately the same ground as the paper, but dealt with the material quite differently and in a much less formal manner.

The appearance of the paper seems to have produced little manifest effect in Vienna or Germany. In France, on the other hand, as Freud reports to Fliess in a letter of July 10, 1893 (1950*a*, Letter 13), it was favourably noticed by Janet, whose resistance to Freud's ideas was only to develop later. Janet included a long and highly laudatory account of the 'Preliminary Communication' in a paper on 'Some Recent Definitions of Hysteria' published in the *Archives de Neurologie* in June and July 1893. He used this paper as the final chapter of his book, *L'état mental des hystériques*, published in 1894. More unexpected,

perhaps, is the fact that in April 1893—only three months after the publication of the 'Preliminary Communication'—a fairly full account of it was given by F. W. H. Myers at a general meeting of the Society for Psychical Research in London and was printed in their *Proceedings* in the following June. The 'Preliminary Communication' was also fully abstracted and discussed by Michell Clarke in *Brain* (1894, 125). The most surprising and unexplained reaction, however, was the publication in February and March 1893, in the *Gaceta médica de Granada* (**11**, 105–11 and 129–35), of a complete translation of the 'Preliminary Communication', in Spanish.

The authors' next task was the preparation of the case material, and already on February 7, 1894, Freud spoke of the book as 'half-finished: what remains to be done is only a small minority of the case histories and two general chapters'. In an unpublished passage in the letter of May 21 he mentions that he is just writing the last case history, and on June 22 (1950*a*, Letter 19) he gives a list of what 'the book with Breuer' is to contain: 'five case histories, an essay by him, with which I have nothing at all to do, on the theories of hysteria (summarizing and critical), and one by me on therapy which I have not started yet'. After this there was evidently a hold-up, for it is not until March 4, 1895 (ibid., Letter 22) that he writes to say that he is 'hurriedly working at the essay on the therapy of hysteria', which was finished by March 13 (unpublished letter). In another unpublished letter, of April 10, he sends Fliess the second half of the proofs of the book, and next day tells him it will be out in three weeks.

The *Studies on Hysteria* seem to have been duly published in May 1895, though the exact date is not stated. The book was unfavourably received in German medical circles; it was, for instance, very critically reviewed by Adolf von Strümpell, the well-known neurologist (*Deutsch. Z. Nervenheilk.*, 1896, 159). On the other hand, a non-medical writer, Alfred von Berger, later director of the Vienna Burgtheater, wrote appreciatively of it in the *Neue Freie Presse* (February 2, 1896). In England it was given a long and favourable notice in *Brain* (1896, 401) by Michell Clarke, and once again Myers showed his interest in it in an address of considerable length, first given in March 1897, which was ultimately included in his *Human Personality* (1903).

It was more than ten years before there was a call for a second edition of the book, and by that time the paths of its two authors had diverged. In May 1906 Breuer wrote to Freud agreeing on a reprint, but there was some discussion about whether a new joint preface was desirable. Further delays followed, and in the end, as will be seen below, two separate prefaces were written. These bear the date of July 1908, though the second edition was not actually published till 1909. The text was unaltered in this and the later editions of the book. But in 1924 Freud wrote some additional footnotes for the volume of his collected works containing his share of the *Studies* (published in 1925) and made one or two small changes in the text.

(2)

THE BEARING OF THE STUDIES ON PSYCHO-ANALYSIS

The *Studies on Hysteria* are usually regarded as the starting-point of psycho-analysis. It is worth considering briefly whether and in what respects this is true. For the purposes of this discussion the question of the shares in the work attributable to the two authors will be left on one side for consideration below, and the book will be treated as a whole. An enquiry into the bearing of the *Studies* upon the subsequent development of psycho-analysis may be conveniently divided into two parts, though such a separation is necessarily an artificial one. To what extent and in what ways did the technical procedures described in the *Studies* and the clinical findings to which they led pave the way for the practice of psycho-analysis? To what extent were the theoretical views propounded here accepted into Freud's later doctrines?

The fact is seldom sufficiently appreciated that perhaps the most important of Freud's achievements was his invention of the first instrument for the scientific examination of the human mind. One of the chief fascinations of the present volume is that it enables us to trace the early steps of the development of that instrument. What it tells us is not simply the story of the overcoming of a succession of obstacles; it is the story of the *discovery* of a succession of obstacles that have to be overcome.

Breuer's patient Anna O. herself demonstrated and overcame the first of these obstacles—the amnesia characteristic of the hysterical patient. When the existence of this amnesia was brought to light, there at once followed a realization that the patient's manifest mind was not the whole of it, that there lay behind it an *unconscious* mind (p. 45 ff.). It was thus plain from the first that the problem was not merely the investigation of *conscious* mental processes, for which the ordinary methods of enquiry used in everyday life would suffice. If there were also *unconscious* mental processes, some special instrument was clearly required. The obvious instrument for this purpose was hypnotic suggestion—hypnotic suggestion used, not for directly therapeutic purposes, but to persuade the patient to produce material from the unconscious region of the mind. With Anna O. only slight use of this instrument seemed necessary. She produced streams of material from her 'unconscious', and all Breuer had to do was to sit by and listen to them without interrupting her. But this was not so easy as it sounds, and the case history of Frau Emmy shows at many points how difficult it was for Freud to adapt himself to this new use of hypnotic suggestion and to listen to all that the patient had to say without any attempt at interference or at making short cuts (e.g. pp. 60 *n.* and 62 *n.* 1). Not all hysterical patients, moreover, were so amenable as Anna O.; the deep hypnosis into which she fell, apparently of her own accord, was not so readily obtained with everyone. And here came a further obstacle: Freud tells us that he was far from being an adept at hypnotism. He gives us several accounts in this book (e.g. p. 107 ff.) of how he circumvented this difficulty, of how he gradually gave up his attempts at bringing about hypnosis and contented himself with putting his patients into a state of 'concentration' and with the occasional use of pressure on the forehead. But it was the abandonment of hypnotism that widened still further his insight into mental processes. It revealed the presence of yet another obstacle—the patients' 'resistance' to the treatment pp. 154 and 268 ff.), their unwillingness to co-operate in their own cure. How was this unwillingness to be dealt with? Was it to be shouted down or suggested away? Or was it, like other mental phenomena, simply to be investigated? Freud's choice of this second path led him directly into the uncharted world which he was to spend his whole life in exploring.

In the years immediately following the *Studies* Freud abandoned more and more of the machinery of deliberate suggestion [cf. p. 110 *n.*] and came to rely more and more on the patient's flow of 'free associations'. The way was opened up to the analysis of dreams. Dream-analysis enabled him, in the first place, to obtain an insight into the workings of the 'primary process' in the mind and the ways in which it influenced the products of our more accessible thoughts, and he was thus put in possession of a new technical device—that of 'interpretation'. But dream-analysis made possible, in the second place, his own self-analysis, and his consequent discoveries of infantile sexuality and the Oedipus complex. All these things, apart from some slight hints,[1] still lay ahead. But he had already, in the last pages of this volume, come up against one further obstacle in the investigator's path—the 'transference' (p. 301 ff.). He had already had a glimpse of its formidable nature and had even, perhaps, already begun to recognize that it was to prove not only an obstacle but also another major instrument of psycho-analytic technique.

The main theoretical position adopted by the authors of the 'Preliminary Communication' seems, on the surface, a simple one. They hold that, in the normal course of things, if an experience is accompanied by a large amount of 'affect', that affect is either 'discharged' in a variety of conscious reflex acts or becomes gradually worn away by association with other conscious mental material. In the case of hysterical patients, on the other hand (for reasons which we shall mention in a moment), neither of these things happens. The affect remains in a 'strangulated' state, and the memory of the experience to which it is attached is cut off from consciousness. The affective memory is thereafter manifested in hysterical symptoms, which may be regarded as 'mnemic symbols'—that is to say as symbols of the suppressed memory (p. 90). Two principal reasons are suggested to explain the occurrence of this pathological outcome. One is that the original experience took place while the subject was in a particular dissociated state of mind described as 'hypnoid'; the other is that the experience was one which the subject's 'ego' regarded as 'incompatible' with itself

[1] See, for instance, the remarks on dreams in a footnote on p. 69 and a hint at the notion of free association on p. 56.

and which had therefore to be 'fended off'. In either case the therapeutic effectiveness of the 'cathartic' procedure is explained on the same basis: if the original experience, along with its affect, can be brought into consciousness, the affect is by that very fact discharged or 'abreacted', the force that has maintained the symptom ceases to operate, and the symptom itself disappears.

This all seems quite straightforward, but a little reflection shows that much remains unexplained. Why should an affect need to be 'discharged'? And why are the consequences of its not being discharged so formidable? These underlying problems are not considered at all in the 'Preliminary Communication', though they had been alluded to briefly in two of the posthumously published drafts (1941*a* and 1940*d*) and a hypothesis to provide an explanation of them was already in existence. Oddly enough, this hypothesis was actually stated by Freud in his lecture of January 11, 1893 (see p. xiv), in spite of its omission from the 'Preliminary Communication' itself. He again alluded to it in the last two paragraphs of his first paper on 'The Neuro-Psychoses of Defence' (1894*a*), where he specifically states that it underlay the theory of abreaction in the 'Preliminary Communication' of a year earlier. But this basic hypothesis was first formally produced and given a name in 1895 in the second section of Breuer's contribution to the present volume (p. 192 ff.). It is curious that this, the most fundamental of Freud's theories, was first fully discussed by Breuer (attributed by him, it is true, to Freud), and that Freud himself, though he occasionally reverted to its subject-matter (as in the early pages of his paper on 'Instincts and their Vicissitudes', 1915*c*), did not mention it explicitly till he wrote *Beyond the Pleasure Principle* (1920*g*). He did, as we now know, refer to the hypothesis by name in a communication to Fliess of uncertain date, possibly 1894 (Draft D, 1950*a*), and he considered it fully, though under another name (see below, p. xxiv), in the 'Project for a Scientific Psychology' which he wrote a few months after the publication of the *Studies*. But it was not until fifty-five years later (1950*a*) that Draft D and the 'Project' saw the light of day.

The 'principle of constancy' (for this was the name given to the hypothesis) may be defined in the terms used by Freud himself in *Beyond the Pleasure Principle*: 'The mental apparatus

endeavours to keep the quantity of excitation present in it as low as possible or at least to keep it constant.' (*Standard Ed.*, **18**, 9.) Breuer states it below (p. 197) in very similar terms, but with a neurological twist, as 'a tendency to keep intra-cerebral excitation constant'.[1] In his discussion on p. 201 ff., he argues that the affects owe their importance in the aetiology of hysteria to the fact that they are accompanied by the production of large quantities of excitation, and that these in turn call for discharge in accordance with the principle of constancy. Similarly, too, traumatic experiences owe their pathogenic force to the fact that they produce quantities of excitation too large to be dealt with in the normal way. Thus the essential theoretical position underlying the *Studies* is that the clinical necessity for abreacting affect and the pathogenic results of its becoming strangulated are explained by the much more general tendency (expressed in the principle of constancy) to keep the quantity of excitation constant.

It has often been thought that the authors of the *Studies* attributed the phenomena of hysteria only to traumas and to ineradicable memories of them, and that it was not until later that Freud, after shifting the emphasis from infantile traumas to infantile phantasies, arrived at his momentous 'dynamic' view of the processes of the mind. It will be seen, however, from what has just been said, that a dynamic hypothesis in the shape of the principle of constancy already underlay the theory of trauma and abreaction. And when the time came for widening the horizon and for attributing a far greater importance to instinct as contrasted with experience, there was no need to modify the basic hypothesis. Already, indeed, Breuer points out the part played by 'the organism's major physiological needs and instincts' in causing increases in excitation which call for discharge (p. 199), and emphasizes the importance of the 'sexual instinct' as 'the most powerful source of persisting increases of excitation (and consequently of neuroses)' (p. 200). Moreover the whole notion of conflict and the repression of

[1] Freud's statement of the principle in the lecture of January 11, 1893, was as follows: 'If a person experiences a psychical impression, something in his nervous system which we will for the moment call the "sum of excitation" is increased. Now in every individual there exists a tendency to diminish this sum of excitation once more, in order to preserve his health . . .' (Freud, 1893 *h*.).

incompatible ideas is explicitly based on the occurrence of unpleasurable increases of excitation. This leads to the further consideration that, as Freud points out in *Beyond the Pleasure Principle* (*Standard Ed.*, **18**, 7 ff.), the 'pleasure principle' itself is closely bound up with the principle of constancy. He even goes further and declares (ibid., 62) that the pleasure principle 'is a tendency operating in the service of a function whose business it is to free the mental apparatus entirely from excitation or to keep the amount of excitation in it constant or to keep it as low as possible.' The 'conservative' character which Freud attributes to the instincts in his later works, and the 'compulsion to repeat', are also seen in the same passage to be manifestations of the principle of constancy; and it becomes clear that the hypothesis on which these early *Studies on Hysteria* were based was still being regarded by Freud as fundamental in his very latest speculations.

(3)

The Divergences between the Two Authors

We are not concerned here with the personal relations between Breuer and Freud, which have been fully described in the first volume of Ernest Jones's biography; but it will be of interest to discuss briefly their *scientific* differences. The existence of such differences was openly mentioned in the preface to the first edition, and they were often enlarged upon in Freud's later publications. But in the book itself, oddly enough, they are far from prominent; and even though the 'Preliminary Communication' is the only part of it with an explicitly joint authorship, it is not easy to assign with certainty the responsibility for the origin of the various component elements of the work as a whole.

We can no doubt safely attribute to Freud the later technical developments, together with the vital theoretical concepts of resistance, defence and repression which arose from them. It is easy to see from the account given on p. 268 ff. how these concepts followed from the replacement of hypnosis by the pressure technique. Freud himself, in his 'History of the Psycho-Analytic Movement' (1914*d*), declares that 'the theory of repression is the foundation stone on which the structure of

psycho-analysis rests', and gives the same account as he does here of the way in which it was arrived at. He also asserts his belief that he reached this theory independently, and the history of the discovery amply confirms that belief. He remarks in the same passage that a hint at the notion of repression is to be found in Schopenhauer (1844), whose works, however, he read only late in life; and it has recently been pointed out that the word '*Verdrängung*' ('repression') occurs in the writings of the early nineteenth century psychologist Herbart (1824) whose ideas carried great weight with many of those in Freud's environment, and particularly with his immediate teacher in psychiatry, Meynert. But no such suggestions detract in any significant degree from the originality of Freud's theory, with its empirical basis, which found its first expression in the 'Preliminary Communication' (p. 10).

As against this, there can be no question that Breuer originated the notion of 'hypnoid states', to which we shall return shortly, and it seems possible that he was responsible for the terms 'catharsis' and 'abreaction'.

But many of the theoretical conclusions in the *Studies* must have been the product of discussions between the two authors during their years of collaboration, and Breuer himself comments (pp. 185–6) on the difficulty of determining priority in such cases. Apart from the influence of Charcot, on which Freud never ceased insisting, it must be remembered, too, that both Breuer and Freud owed a fundamental allegiance to the school of Helmholz, of which their teacher, Ernst Brücke, was a prominent member. Much of the underlying theory in the *Studies on Hysteria* is derived from the doctrine of that school that all natural phenomena are ultimately explicable in terms of physical and chemical forces.[1]

We have already seen (p. xix) that, though Breuer was the first to mention the 'principle of constancy' by name, he attributes the hypothesis to Freud. He similarly attaches Freud's name to the term 'conversion', but (as is explained below, p. 206 *n.*) Freud himself has declared that this applies only to the *word* and that the concept was arrived at jointly. On the other hand there are a number of highly important concepts which seem to be properly attributable to Breuer: the notion of hallucination being a 'retrogression' from imagery to perception (p. 189), the thesis that the functions of perception and memory cannot be performed by the same apparatus (pp. 188–9 *n.*), and finally, and most surprisingly, the distinction between bound (tonic) and unbound (mobile) psychical energy and the correlated distinction between primary and secondary psychical processes (p. 194 *n.*).

[1] The various influences that may possibly have played a part in determining Freud's views are very fully discussed by Ernest Jones (1953, **1**, 44 ff. and 407 ff.). In addition to the names referred to in the text above, special mention should be made of the psycho-physicist Fechner, to whom Freud himself acknowledged his indebtedness in the fifth chapter of his *Autobiographical Study* (1925*d*).

The use of the term '*Besetzung*' ('cathexis'), which makes its first appearance on p. 89 in the sense that was to become so familiar in psycho-analytic theory, is probably to be attributed to Freud. The idea of the whole or a part of the mental apparatus carrying a charge of energy is, of course, presupposed by the principle of constancy. And though the actual term that was to be the standard one first came into use in this volume, the idea had been expressed earlier by Freud in other forms. Thus we find him using such phrases as '*mit Energie ausgestattet*' ('supplied with energy') (1895*b*), '*mit einer Erregungssumme behaftet*' ('loaded with a sum of excitation') (1894*a*), '*munie d'une valeur affective*' ('provided with a quota of affect') (1893*c*), '*Verschiebungen von Erregungssummen*' ('displacements of sums of excitation') (1941*a* [1892]) and, as long ago as in his preface to his first translation of Bernheim (1888–9), '*Verschiebungen von Erregbarkeit im Nervensystem*' ('displacements of excitability in the nervous system').

But this last quotation is a reminder of something of great importance that may very easily be overlooked. There can be no doubt that at the time of the publication of the *Studies* Freud regarded the term 'cathexis' as a purely physiological one. This is proved by the definition of the term given by him in Part I, Section 2, of his 'Project for a Scientific Psychology' by which (as is shown in the Fliess letters) his mind was already occupied, and which was written only a few months later. There, after giving an account of the recently discovered neurological entity, the 'neurone', he goes on: 'If we combine this account of neurones with an approach on the lines of the quantity theory, we arrive at the idea of a "cathected" neurone, filled with a certain quantity, though at other times it may be empty.'

The neurological bias of Freud's theories at this period is further shown by the form in which the principle of constancy is stated in the same passage in the 'Project'. It is given the name of 'the principle of neuronic inertia' and is defined as asserting 'that neurones tend to divest themselves of quantity'. A remarkable paradox is thus revealed. Breuer, as will be seen (p. 185), declares his intention of treating the subject of hysteria on purely psychological lines: 'In what follows little mention will be made of the brain and none whatever of molecules. Psychical processes will be dealt with in the language of psychology.' But in fact his theoretical chapter is largely concerned with 'intracerebral excitations' and with parallels between the nervous system and electrical installations. On the other hand Freud was devoting all his energies to explaining mental phenomena in physiological and chemical terms. Nevertheless, as he himself somewhat ruefully confesses (p. 160), his case histories read like short stories and his analyses are psychological ones.

The truth is that in 1895 Freud was at a half-way stage in the process of moving from physiological to psychological explanations of psychopathological states. On the one hand he was proposing what was broadly speaking a chemical explanation of the 'actual' neuroses—neurasthenia and anxiety neurosis—(in his two papers on anxiety neurosis, 1895*b* and 1895*f*), and on the other hand he was proposing an essentially psychological explanation—in terms of 'defence' and 'repression'—of hysteria and obsessions (in his two papers on 'The Neuro-Psychoses of Defence', 1894*a* and 1896*b*). His earlier training and career as a neurologist led him to resist the acceptance of psychological explanations as ultimate; and he was engaged in devising a complicated structure of hypotheses intended to make it possible to describe mental events in purely neurological terms. This attempt culminated in the 'Project' and was not long afterwards abandoned. To the end of his life, however, Freud continued to adhere to the chemical aetiology of the 'actual' neuroses and to believe that a physical basis for all mental phenomena might ultimately be found. But in the meantime he gradually came round to the view expressed by Breuer that psychical processes can only be dealt with in the language of psychology. It was not until 1905 (in his book on jokes, Chapter V) that he first explicitly repudiated all intention of using the term 'cathexis' in any but a psychological sense and all attempts at equating nerve-tracts or neurones with paths of mental association.[1]

What, however, were the essential scientific differences between Breuer and Freud? In his *Autobiographical Study* (1925*d*) Freud says that the first of these related to the aetiology of hysteria and could be described as 'hypnoid states versus neuroses of defence'. But once again, in this volume itself the issue is less clear-cut. In the joint 'Preliminary Communication' both aetiologies are accepted (p. 10 f.). Breuer, in his theoretical chapter, evidently lays most emphasis on hypnoid states (p. 215 ff.), but he also stresses the importance of 'defence' (pp. 214 and 235–6), though a little half-heartedly. Freud seems to accept the notion of 'hypnoid states' in his 'Katharina' case history (p. 128) [2] and, less definitely, in that of Frau Elisabeth (p. 167 *n.*). It is only in his final chapter that his scepticism begins to be apparent (p. 286). In a paper on 'The Aetiology of Hysteria' published in the following year (1896*c*) this scepticism is still more openly expressed, and in a footnote to his 'Dora' case history (1905*e*) he declares that the term 'hypnoid states' is 'superfluous and misleading' and that the hypothesis 'sprang entirely from the initiative of Breuer' (*Standard Ed.*, **7**, 27 *n.*).

But the chief difference of opinion between the two authors upon which Freud later insisted concerned the part played by sexual impulses in the causation of hysteria. Here too, however, the *expressed* difference will be found less clear than would be expected. Freud's belief in the sexual origin of hysteria can be inferred plainly enough from the discussion in his chapter on psychotherapy (p. 257 ff.), but he nowhere asserts, as he was later to do, that in cases of hysteria a sexual aetiology was

[1] The insecurity of the neurological position which Freud was still trying to maintain in 1895 is emphasized by the correction that he felt obliged to make thirty years later in the very last sentence of the book. In 1895 he used the word '*Nervensystem*' ('nervous system'); in 1925 he replaced it by '*Seelenleben*' ('mental life'). Yet what was ostensibly a momentous change did not in the least affect the meaning of the sentence. The old neurological vocabulary had already been no more than a husk at the time when Freud penned the words.

[2] As he already had in his first paper on 'The Neuro-Psychoses of Defence' (1894*a*) and in the memorandum 'III' (1941*b*), almost certainly written in 1892 (see above p. xiv).

invariably present.[1] On the other hand, Breuer speaks at several points in the strongest terms of the importance of the part played by sexuality in the neuroses, particularly in the long passage on pp. 245–7. He says, for instance (as has already been remarked, p. xx), that 'the sexual instinct is undoubtedly the most powerful source of persisting increases of excitation (and consequently of neuroses)' (p. 200), and declares (p. 246) that 'the great majority of severe neuroses in women have their origin in the marriage bed'.

It seems as though, in order to find a satisfactory explanation of the dissolution of this scientific partnership, we should have to look behind the printed words. Freud's letters to Fliess show Breuer as a man full of doubts and reservations, always insecure in his conclusions. There is an extreme instance of this in a letter of November 8, 1895 (1950*a*, Letter 35), about six months after the publication of the *Studies*: 'Not long ago Breuer made a big speech about me at the Doktorenkollegium, in which he announced his conversion to belief in the sexual aetiology [of the neuroses]. When I took him on one side to thank him for it, he destroyed my pleasure by saying: "All the same I don't believe it." Can you understand that? I can't.' Something of the kind can be read between the lines of Breuer's contributions to the *Studies*, and we have the picture of a man half-afraid of his own remarkable discoveries. It was inevitable that he should be even more disconcerted by the premonition of still more unsettling discoveries yet to come; and it was inevitable that Freud in turn should feel hampered and irritated by his yoke-fellow's uneasy hesitations.

It would be tedious to enumerate the many passages in Freud's later writings in which he refers to the *Studies on Hysteria* and to Breuer; but a few quotations will illustrate the varying emphasis in his attitude to them.

In the numerous short accounts of his therapeutic methods and psychological theories which he published during the years immediately succeeding the issue of the *Studies* he was at pains to bring out the differences between 'psycho-analysis' and the cathartic method—the technical innovations, the extension of his procedure to neuroses other than hysteria, the establishment of the motive of 'defence', the insistence on a sexual aetiology and, as we have already seen, the final rejection of 'hypnoid states'. When we reach the first series of Freud's major works—the volumes on dreams (1900*a*), on parapraxes (1901*b*), on jokes (1905*c*) and on sexuality (1905*d*)—there is naturally little or no retrospective material; and it is not until the five lectures at Clark University (1910*a*) that we find any extensive historical survey. In those lectures Freud appeared anxious to establish the continuity between his work and Breuer's. The whole of the first lecture and much of the second are devoted to a summary of the *Studies*, and the impression given was that not Freud but Breuer was the true founder of psycho-analysis.

[1] Indeed, in the fourth of his *Five Lectures* (1910*a*), he categorically asserts that at the time of the publication of the *Studies* he did not yet believe that this was so.

The next long retrospective survey, in the 'History of the Psycho-Analytic Movement' (1914*d*), was in a very different key. The whole paper, of course, was polemical in its intent and it is not surprising that in sketching the early history of psycho-analysis Freud stressed his differences from Breuer rather than his debts to him, and that he explicitly retracted his view of him as the originator of psycho-analysis. In this paper, too, Freud dilated on Breuer's inability to face the sexual transference and revealed the 'untoward event' which ended the analysis of Anna O. (pp. 40–1 *n*.).

Next came what seems almost like an *amende*—it has already been mentioned on p. xxiii—the unexpected attribution to Breuer of the distinction between bound and unbound psychical energy and between the primary and secondary processes. There had been no hint of this attribution when these hypotheses were originally introduced by Freud (in *The Interpretation of Dreams*); it was first made in a footnote to Section V of the metapsychological paper on 'The Unconscious' (1915*e*) and repeated in *Beyond the Pleasure Principle* (1920*g*; *Standard Ed.*, **18**, 26–7 and 31). Not long after the last of these there were some appreciative sentences in an article contributed by Freud to Marcuse's *Handwörterbuch* (1923*a*; *Standard Ed.*, **18**, 236): 'In a theoretical section of the *Studies* Breuer brought forward some speculative ideas about the processes of excitation in the mind. These ideas determined the direction of future lines of thought . . .' In somewhat the same vein Freud wrote a little later in a contribution to an American publication (1924*f*): 'The cathartic