

The seduction theory*

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In 1896 Freud presented a theory about hysteria which is generally known as the seduction theory. According to the 'seduction theory', hysterical patients suffer from long-term effects of sexual assaults during childhood. After 1896 Freud changed his mind and developed new and quite different theories about hysteria.

Six years ago Jean Schimek published an article about the seduction theory which has not received the attention it deserves. She showed in detail that Freud in later years gave descriptions of the content of the seduction theory which were radically different from its initial content. The goal of her article was to show that the seduction theory was different from the prevailing view of it. She did not, however, imply that her evidence and arguments, which anticipated our own, suggest doubts concerning Freud's integrity as a theorist. In this article we will try to do what Schimek did not do.

In the mid-eighties, the former psychoanalyst Jeffrey Masson and his book on Freud's seduction theory got considerable attention. Freud's 1896 emphasis on the long-term effects of sexual child-abuse has certain similarities with ideas of modern, especially feminist, authors on sexual violence against children. Masson's sympathy with those ideas led him to ask why Freud had abandoned his seduction theory. According to Masson, Freud abandoned the theory, not because it was untrue, but because Freud could not bear the disapproval of his medical colleagues. Masson's former colleagues, the psychoanalysts, disapproved of Masson's ideas. They argued that – whatever might have been the reasons for Freud abandoning his seduction theory – he certainly was not motivated by a wish to please his colleagues, because his new theory, with its emphasis on innate childhood sexuality, was at least as repellent. We wish to resolve this debate and show why Freud abandoned the seduction theory.

According to Masson, women who had been sexually abused in childhood,

* The argument of this article is to appear in a book by Han Israëls entitled *The Freud Case*.

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and who still suffered the consequences of these traumas, came to Freud, conquered their feelings of shame and found the courage to tell Freud what had happened to them. Freud was – according to Masson – shocked by these stories of his so-called hysterical patients, and published them. These publications were badly received. The medical establishment did not want to hear how common sexual abuse of children was. Freud became more and more isolated. Then, gradually, through a mainly unconscious development, Freud began to succumb to the dominant medical opinions; he began to doubt the truth of the stories his patients were telling him and to think that these stories were largely fantasies resulting from the sexual longings of the patients themselves. That is how Freud ‘discovered’ the Oedipus complex. This is, in a nutshell, Masson’s view.

In the heat of the debate between Masson and his psychoanalytic opponents very few people have noticed that Masson attributed to Freud a version of the seduction theory that has never existed.¹ According to Masson, Freud believed the stories his patients told him about their childhood traumas. However, in Freud’s own 1896 version, no patients told him such stories. One essential aspect of Freud’s 1896 seduction theory is different from what Masson wants us to believe: in 1896 Freud thought that his hysterical patients suffered from unconscious memories of sexual abuse in their early childhood. According to Freud the patients themselves did not consciously know that they had been sexually abused.

Consider for example how Freud in 1896 defended himself against the objection that these infantile sexual experiences might be only fantasies of his patients:

The behaviour of patients while they are reproducing these infantile experiences is in every respect incompatible with the assumption that the scenes [the traumatic experiences] are anything else than a reality which is being felt with distress and reproduced with the greatest reluctance. Before they come for analysis the patients know nothing about these scenes. They are indignant as a rule if we warn them that such scenes are going to emerge. Only the strongest compulsion of the treatment can induce them to embark on a reproduction of them. While they are recalling these infantile experiences to consciousness, they suffer under the most violent sensations, of which they are ashamed and which they try to conceal; and, even after they have gone through them once more in such a convincing manner, they still attempt to withhold belief from them, by emphasizing the fact that, unlike what happens in the case of other forgotten material, they have no feeling of remembering the scenes. (Freud 1896c: 204)

This is quite different from Masson’s summary of Freud’s theory:

Freud’s female patients had the courage to face what had happened to them in childhood – often this included violent scenes of rape by a father – and to

¹ As far as we know, Frank Cioffi (1984) was the first to notice this.

communicate their traumas to Freud, no doubt hesitating to believe their own memories and reluctant to remember the deep shame and hurt they had felt. Freud listened and understood and gave them permission to remember and speak of these terrible events. Freud did not think they were fantasies: 'Doubts about the genuineness of the infantile sexual scenes can, however, be deprived of their force here and now by more than one argument. In the first place, the behaviour of patients while they are reproducing these infantile experiences is in every respect incompatible with the assumption that the scenes are anything else than a reality which is being felt with distress and reproduced with the greatest reluctance.' (Masson 1984: 9, 10)

Here Masson ended his Freud quotation. We just saw how it continued: how Freud compelled his patients to 'reproduce' infantile sexual traumas in which the patients themselves did not believe. However, Masson could not quote that, because it explicitly contradicted Masson's allegation that Freud 'listened and understood and gave them [his female patients] permission to remember and speak of these terrible events'.

Here is another Freud quotation that proves that in 1896 he thought that people became hysterical because they do not remember having been sexually abused in early childhood; here he was defending his theory against an objection:

We have heard and have acknowledged that there are numerous people who have a very clear recollection of infantile sexual experiences and who nevertheless do not suffer from hysteria. This objection has no weight; but it provides an occasion for making a valuable comment. According to our understanding of the neurosis, people of this kind *ought* not to be hysterical at all, or at any rate, not hysterical as a result of the scenes which they consciously remember. With our patients, those memories are never conscious; but we cure them of their hysteria by transforming their unconscious memories of the infantile scenes into conscious ones. (Freud 1896c: 211)

Apparently Freud thought it unnecessary to reconcile the end of this quotation – that he had transformed unconscious memories into conscious ones – with the earlier quotation from his same article saying that after treatment his patients still had had no feelings of remembering the infantile sexual scenes. Did Freud's patients after treatment have conscious memories of these events, or did they not? If they did not, in what sense did Freud 'cure' them? We shall see that there is reason to doubt that Freud actually did cure his patients.

Freud: a similar distortion

The version of Freud's seduction theory that Masson deals with is the same distorted version promulgated long before by Freud himself, when in later years he recalled his seduction theory. For example, in 1933 he wrote:

You will recall an interesting episode in the history of analytic research which caused me many distressing hours. In the period in which the main interest

was directed to discovering infantile sexual traumas, almost all my women patients told me that they had been seduced by their father. I was driven to recognize in the end that these reports were untrue and so came to understand that hysterical symptoms are derived from fantasies and not from real occurrences. (Freud 1933: 120)

Here Freud alleged that in 1896 he had believed the stories of his female patients of how they had been 'seduced'. A similar distortion can be found in Freud's *Autobiographical Study* of 1925:

Under the influence of the technical procedure which I used at that time, the majority of my patients reproduced from their childhood scenes in which they were sexually seduced by some grown-up person. With female patients the part of seducer was almost always assigned to their father. I believed these stories, and consequently supposed that I had discovered the roots of the subsequent neurosis in these experiences of sexual seduction in childhood. My confidence was strengthened by a few cases in which relations of this kind with a father, uncle, or elder brother had continued up to an age at which memory was to be trusted. If the reader feels inclined to shake his head at my credulity, I cannot altogether blame him; though I may plead that this was at a time when I was intentionally keeping my critical faculty in abeyance so as to preserve an unprejudiced and receptive attitude towards the many novelties which were coming to my notice every day. When, however, I was at last obliged to recognize that these scenes of seduction had never taken place, and that they were only fantasies which my patients had made up or which I myself had perhaps forced on them, I was for some time completely at a loss. [. . .] I do not believe even now that I forced the seduction-fantasies on my patients, that I 'suggested' them. (Freud 1925: 33, 34)

Here Freud described himself as someone who, in his innocence, had simply believed the fantastic stories of his patients. However, the quotations from 1896 given earlier told a different story; in 1896 Freud had written that his patients did not remember having been 'seduced'.

In later years Freud presented his 1896 theory as naïve, which could account for his having gone on to abandon it. Did Freud have to distort his earlier theory in order to explain his abandonment of it? If so, why? Why could he not give a correct summary of his earlier theory and then explain his change of mind?

Masson presented himself as an admirer of an early theory of Freud which Freud in fact had never embraced. However, Freud's distortion showed that Masson did have a point when he wondered what exactly were the true reasons for Freud's abandonment of his seduction theory. This will be the central issue of this article.

THE SEDUCTION THEORY: A SUMMARY

Freud presented his seduction theory in three articles, all published in 1896. We will use here only the last of these articles, which is the most extensive and

reports about more patients than the other two. Freud wrote this article, called 'The Aetiology of Hysteria', on the basis of a talk he had given in the spring of 1896 for an association of Viennese neurologists and psychiatrists. The article was published in five successive instalments of a medical weekly.

How Freud arrived at his new theory

Freud started his article with two instalments entirely devoted to a sketch of how he had arrived at his new ideas. The sketch began with a long summary of his earlier views on hysteria, as he and Josef Breuer had presented them a year before in their *Studies on Hysteria*. According to Breuer, a hysterical symptom disappears when the patient remembers, with all the accompanying emotions, the occasion when that specific symptom had started. One would expect – so Freud wrote – that this is the way to discover the causes of hysteria:

This expectation proves true; and it cannot fail to, since Breuer's theses, when put to the test in a considerable number of cases, have turned out to be correct. But the path from the symptoms of hysteria to its aetiology is more laborious and leads through other connections than one would have imagined. (Freud 1896c: 193)

Freud then alleged that a hysterical symptom can be explained by a traumatic experience only if the content of the trauma is similar to the content of the hysterical symptom, and if the trauma is sufficiently strong. Now most of the traumas discovered by Breuer were not like that: they often were quite trivial, and the content often was quite different from the hysterical symptom.

Moreover, Gentlemen, this first disappointment we meet with in following Breuer's method is immediately succeeded by another, and one that must be especially painful to us as physicians. When our procedure leads, as in the cases described above, to findings which are insufficient as an explanation both in respect to their suitability as determinants [by 'determinants' Freud meant that the content of the trauma should 'determine' the content of the symptom] and to their traumatic effectiveness [by 'traumatic effectiveness' Freud meant that the trauma is sufficiently strong], we also fail to secure any therapeutic gain; the patient retains his symptoms unaltered, in spite of the initial result yielded by the analysis. (Freud 1896c: 195)

This was something new: a year before, Freud and Breuer had alleged that a hysterical symptom vanishes as soon as the patient relives the occasion on which the symptom started, even if the occasion was a trivial one. Apparently Freud still thought that Breuer's ideas were basically correct, although in practice they did not work. Whatever 'the initial result yielded by the analysis' was, it seems that Breuer's statements needed revising.

Given that Breuer's theory was therapeutically ineffective, one will understand

how great the temptation is at this point to proceed no further with what is in any case a laborious piece of work. But perhaps all we need is a new idea in

order to help us out of our dilemma and lead to valuable results. The idea is this. As we know from Breuer, hysterical symptoms can be resolved if, starting from them, we were able to find the path back to the memory of a traumatic experience. If the memory which we have uncovered does not answer our expectations, it may be that we ought to pursue the same path a little further; perhaps behind the first traumatic scene there may be concealed the memory of a second, which satisfies our requirements better and whose reproduction has a greater therapeutic effect; so that the scene that was first discovered only has the significance of a connecting link in the chain of associations. (Freud 1896c: 195)

And maybe – so Freud continued – behind the second scene there is a third, and so on.

Well, Gentlemen, this supposition is correct. If the first-discovered scene is unsatisfactory, we tell our patient that this experience explains nothing, but that behind it there must be hidden a more significant, earlier, experience; and we direct his attention by the same technique to the associative thread which connects the two memories – the one that has been discovered and the one that has still to be discovered. (Freud 1896c: 195, 196)

It would be interesting to know what that technique exactly was. How is it possible to direct the attention of the patient to the associative thread that leads to a memory that has not yet been discovered? How did Freud know the direction of that thread? Unfortunately, however, Freud did not describe his technique:

It is no part of my intention to-day to discuss the difficult technique of this therapeutic procedure. (Freud 1896c: 193)

He did present a few examples of how a hysterical symptom can be traced to an earlier event, and how behind that event there is a still earlier event, and so on. However, he did not return to these examples, for a reason which is curious:

I shall not return any further to these examples, for I have to confess that they are not derived from any case in my experience but are inventions of mine. Most probably, too, they are bad inventions. I even regard such solutions of hysterical symptoms as impossible. But I was obliged to make up fictitious examples for several reasons, one of which I can state at once. The real examples are all incomparably more complicated: to relate a single one of them in detail would occupy the whole period of this lecture. [. . .] giving an account of the resolution of a single symptom would in fact amount to the task of relating an entire case history. (Freud 1896c: 196, 197)

Freud summarized his first new finding: a hysterical symptom is never caused only by an event occurring when the symptom started; always, memories of earlier events play a part.

You might suppose that the rare instances in which analysis is able to trace the symptom back direct to a traumatic scene that is thoroughly suitable as a

determinant and possesses traumatic force, and is able, by thus tracing it back, at the same time to remove it (in the way described in Breuer's case history of Anna O.) – you might suppose that such instances must, after all, constitute powerful objections to the general validity of the proposition I have just put forward. It certainly looks so. But I must assure you that I have the best grounds for assuming that even in such instances there exists a chain of operative memories which stretches far back behind the first traumatic scene, *even though* the reproduction of the latter alone may have the result of removing the symptom. (Freud 1896c: 197)

Why was the case of Anna O., whose symptoms completely disappeared without any delving into earlier traumatic events, not an objection against Freud's proposition that symptoms are always caused by earlier traumatic events? Because it was not true when in 1895 Breuer wrote that all symptoms had disappeared, and Freud knew that it was not true (see Jones 1953: 225, Ellenberger 1972, Hirschmüller 1989). So Freud was right when he said that the results of the treatment of Anna O. presented no objection to his new proposition. However, it is also clear why Freud could only 'assure' his public that this was so, and could not explain why he was so sure: he did not want his public to know that what he and Breuer had written in 1895 had been untrue.

Now where did Freud's search for earlier memories lead?

Whatever case and whatever symptom we take as our point of departure, *in the end we infallibly come to the field of sexual experience*. [. . .] From previous experience I can foresee that it is precisely against this assertion or against its universal validity that your contradiction, Gentlemen, will be directed. Perhaps it would be better to say, your *inclination* to contradict; for none of you, no doubt, have as yet any investigations at your disposal which, based upon the same procedure, might have yielded a different result. (Freud 1896c: 199)

We have already seen that Freud did not describe his procedure; later on we shall see that Freud was convinced that nobody else knew this procedure and that thus nobody else was really able to judge it.

If you submit my assertion that the aetiology of hysteria lies in sexual life to the strictest examination, you will find that it is supported by the fact that in some eighteen cases of hysteria I have been able to discover this connection in every single symptom, and, where the circumstances allowed, to confirm it by therapeutic success. (Freud 1896c: 199)

It is our purpose not only to present Freud's seduction theory, but also to try to find out more about these 'some eighteen' cases of hysteria in which Freud was able to trace all the symptoms to sexual traumas and where the correctness of these tracings could be confirmed by therapeutic success 'where the circumstances allowed'.

The sexual events towards which the symptoms eventually lead,

occur for the most part at the same period of life – namely, at puberty. (Freud 1896c: 200)

However, these events

are very different from each other both in *kind* and in *importance*. (Freud 1896c: 200)

In some cases there were severe traumas, such as an attempted rape, but in other cases ‘the experiences are astonishingly trivial.’ (Freud 1896c: 200) And the content of these experiences was not always similar to the content of the hysterical symptom.

It seemed an obvious thing, then, to say to ourselves that we must look for the determinants of these symptoms in yet other experiences, in experiences which went still further back – and that we must, for the second time, follow the saving notion which had earlier led us from the first traumatic scenes to the chains of memories behind them. In doing so, [. . .] we arrive at the period of earliest childhood [. . .] Perhaps the abnormal reaction to sexual impressions which surprises us in hysterical subjects at the age of puberty is quite generally based on sexual experiences of this sort in childhood, in which case those experiences must be of a similar nature to one another, and must be of an important kind. (Freud 1896c: 201, 202)

Here ended the second of the five instalments in which Freud’s article was published.

The third instalment began with the solution and the discovery:

You will no doubt have guessed, Gentlemen, that I should not have carried this last line of thought so far if I had not wanted to prepare you for the idea that it is this line alone which, after so many delays, will lead us to our goal. (Freud 1896c: 202)

And now Freud disclosed what he had discovered:

If we have the perseverance to press on with the analysis into early childhood, as far back as a human memory is capable of reaching, we invariably bring the patient to reproduce experiences which, on account both of their peculiar features and of their relations to the symptoms of his later illness, must be regarded as the aetiology of his neurosis for which we have been looking. These *infantile* experiences are once more *sexual* in content, but they are of a far more uniform kind than the scenes at puberty that had been discovered earlier. It is now [. . .] a question [. . .] of sexual experiences affecting the subject’s own body – of *sexual intercourse* (in the wider sense). (Freud 1896c: 202, 203)

Here we have Freud’s thesis: hysteria is caused by sexual experiences in early childhood. At this point, having presented his alleged discovery in its most rudimentary form, Freud continued:

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 psycho-analysis in spite of the intervening decades. I

believe that this is an important finding, the discovery of a *caput Nili* [source of the Nile] in neuropathology; but I hardly know what to take as a starting-point for a continuation of my discussion of this subject. Shall I put before you the actual material I have obtained from my analyses? Or shall I rather try first to meet the mass of objections and doubts which, as I am surely correct in supposing, have now taken possession of your attention? I shall choose the latter course; perhaps we shall then be able to go over the facts more calmly. (Freud 1896c: 203)

Freud suggested that after meeting objections he would ‘perhaps’ put before us the actual material he had obtained from his analyses, but unfortunately he did not do so. After meeting at length a number of objections, Freud made some rather loosely-connected finishing remarks, and ended the article.

It is remarkable that Freud, after the first brief presentation of his discovery, almost exclusively concentrated on meeting possible objections. One would expect a more precise and extensive description of the ‘discovery’ itself. For example: what exactly were these sexual experiences? Who were the people with whom these children had them? How exactly did Freud bring his patients to ‘reproduce’ these experiences? Freud did not answer these questions, so he was right to suspect that at this point his audience would be sceptical. Freud seemed to suggest that it was now up to the others to prove him wrong, but they could not do so without the information that Freud had failed to provide. More than once Freud pretended that his meeting supposed objections to his theory proved its correctness. In the course of discussing these ‘objections’, Freud now and then casually presented new information which gave a better and more complete view of what he thought he had discovered.

The objections

Freud discussed four possible objections. The first of these was that the traumatic scenes which were reconstructed during the treatment never happened in reality, but either were fantasies of the patients or were imposed upon the patients by Freud. Freud presented *seven* answers to this objection. We have already quoted the first answer:

In the first place, the behaviour of patients while they are reproducing these infantile experiences is in every respect incompatible with the assumption that the scenes are anything else than a reality which is being felt with distress and reproduced with the greatest reluctance. Before they come for analysis the patients know nothing about these scenes. They are indignant as a rule if we warn them that such scenes are going to emerge. Only the strongest compulsion of the treatment can induce them to embark on a reproduction of them. While they are recalling these infantile experiences to consciousness, they suffer under the most violent sensations, of which they are ashamed and which they try to conceal; and even after they have gone through them once more in such a convincing manner, they still attempt to withhold belief from

them, by emphasizing the fact that, unlike what happens in the case of other forgotten material, they have no feeling of remembering the scenes. (Freud 1896c: 204)

Here Freud reported that his hysterical patients themselves knew nothing about these scenes before coming for treatment. During the treatment Freud induced them to let these scenes 'emerge' only with 'the strongest compulsion'. Afterward the patients emphasized that they *still* had no feeling of remembering the scenes. One would like to know what the 'reproduction' of the scenes exactly consisted of.

Freud's second answer was:

It is less easy to refute the idea that the doctor forces reminiscences of this sort on the patient, that he influences him by suggestion to imagine and reproduce them. Nevertheless it appears to me equally untenable. I have never yet succeeded in forcing on a patient a scene I was expecting to find, in such a way that he seemed to be living through it with all the appropriate feelings. Perhaps others may be more successful in this. (Freud 1896c: 204, 205)

Here Freud seemed to be admitting that he tried to force the scenes upon his patients. Maybe, however, he meant that he did not always manage to induce in his patients the appropriate feelings, in which cases he inferred that the specific scenes had never actually taken place. Immediately all kinds of questions arise, such as: Is there indeed such a clear difference between scenes accompanied by and scenes unaccompanied by appropriate feelings? How could Freud be sure that *fantasized* scenes were never accompanied by appropriate feelings? Or, alternatively, could not the scenes have been remembered while the accompanying feelings were not?

Immediately after this second answer Freud wrote:

There are, however, a whole number of other things that vouch for the reality of the² infantile sexual scenes. (Freud 1896c: 205)

Freud presented five more answers. If only one of them were convincing, Freud would be proven right. What exactly does, then, 'vouch for the reality of the sexual scenes'?

In the first place there is the uniformity which they exhibit in certain details, which is a necessary consequence if the preconditions of these experiences are always of the same kind, but which would otherwise lead us to believe that there were secret understandings between the various patients. (Freud 1896c: 205)

The uniformity of the details might of course also result from Freud suggesting the same scenes to all the patients.

² This word 'the' is missing in the Standard Edition.

However, Freud had other evidence to 'vouch for' the reality of the sexual scenes, such as:

patients sometimes describe as harmless events whose significance they obviously do not understand, since they would be bound otherwise to be horrified by them. Or again, they mention details, without laying any stress on them, which only someone of experience in life can understand and appreciate as subtle traits of reality. (Freud 1896c: 205)

We do not know what Freud might have had in mind concerning these details which can only be understood and appreciated 'by someone of experience in life'. By the events about which the patients actually should be 'horrified', Freud must have meant acts of sexual perversion, but he gave no examples. Without examples this answer is unconvincing. If he had some good examples, perhaps they would favour the hypothesis that the patients had information which they did not know consciously.

Freud had still 'another and stronger proof' of the reality of the sexual scenes: 'the relationship of the infantile scenes to the content of the whole of the rest of the case history'. (Freud 1896c: 205) The sexual scenes fit into the rest of the case history like the last pieces of a jigsaw puzzle. Freud did not give any examples of these scenes either, and presented no case history. Without any examples this 'proof' too is unconvincing.

We now have reached Freud's sixth answer:

Without wishing to lay special stress on the point, I will add that in a number of cases therapeutic evidence of the genuineness of the infantile scenes can also be brought forward. There are cases in which a complete or partial cure can be obtained without our having to go as deep as the infantile experiences. And there are others in which no success at all is obtained until the analysis has come to its natural end with the uncovering of the earliest traumas. In the former cases we are not, I believe, secure against relapses; and my expectation is that a complete psycho-analysis implies a radical cure of the hysteria. We must not, however, be led into forestalling the lessons of observation. (Freud 1896c: 205, 206)

These sentences should be read carefully because here Freud described his therapeutic results. Such descriptions are always interesting because it is known that Freud was not always reliable in reporting results. Consider, for example, Freud's first well-known case history: the morphinist Ernst Fleischl whom he tried to cure with cocaine; the treatment ended catastrophically, yet Freud always presented the case as a therapeutic success. See also the famous case of Anna O. which Freud presented as a case cured by the treatment, although he knew that the therapy (by Breuer) actually had come to an end because the patient had had to be transferred to a mental hospital. Now what is striking in the passage just quoted is the modesty and carefulness with which Freud wrote about his amount of therapeutic success. Sometimes, Freud wrote, there is therapeutic success without reaching the infantile experiences, but such success

might be temporary; in other cases there is no success before the earliest traumas have been discovered, and Freud supposed that such a complete psycho-analysis meant a lasting recovery. This would imply that at least all eighteen cases, which Freud had analysed completely, would have been cured completely. However, Freud nowhere made that allegation explicitly. We have already quoted Freud writing that he had been able to confirm his ideas by therapeutic success 'where the circumstances allowed' (Freud 1896c: 199). This lack of special stress on the therapeutic successes is remarkable, because therapeutic successes would have supported Freud's theory. In any event, therapeutic successes are important, even if the theory behind them proves to be wrong.

We have now arrived at Freud's seventh and last answer to the first objection:

There would be one other proof, and a really unassailable one, of the genuineness of the³ childhood sexual experiences – namely, if the statements of someone who is being analysed were to be confirmed by someone else, whether under treatment or not. (Freud 1896c: 206)

Freud had been fortunate enough

that, out of eighteen cases, I have been able to obtain an objective confirmation of this sort in two. In one instance, it was the brother (who had remained well) who of his own accord confirmed – not, it is true, his earliest sexual experiences with his sister (who was the patient) – but at least scenes of that kind from later childhood, and the fact that there had been sexual relations dating further back. (Freud 1896c: 206)

This is not 'a really unassailable proof' that the earliest scenes had really happened – the earliest scenes were not confirmed – but this certainly supported Freud's reconstruction. The second instance was this:

In the other instance, it happened that two women whom I was treating had as children had sexual relations with the same man, in the course of which certain scenes had take place *à trois*. A particular symptom, which was derived from these childhood events, had developed in both women, as evidence of what they had experienced in common. (Freud 1896c: 206)

Did Freud know that as children these two women had had sexual relations? The only supporting evidence he presented was that both women had one particular symptom in common.

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Freud now came to a second objection: some will say that there exists much less

³ Here again the Standard Edition omitted this 'the'.

sexual abuse of children than his theory implies, while others will say that sexual abuse of children is so common that, if Freud were right, there would be much more hysteria.

Is it true that sexual abuse of children is relatively rare? No, Freud said:

It seems to me certain that our children are far more often exposed to sexual assaults than the few precautions taken by parents in this connection would lead us to expect. When I first made enquiries about what was known on the subject, I learnt from colleagues that there are several publications by paediatricians which stigmatize the frequency of sexual practices by nurses and nursery maids, carried out even on infants in arms; and in the last few weeks I have come across a discussion of 'Coitus in Childhood' by Dr Stekel in Vienna (*Wiener medic. Blätter*, 18 April 1896).⁴ I have not had time to collect other published evidence. (Freud 1896c: 207)

It is surprising to see how casually Freud dealt with the incidence of sexual child abuse: he did not have time to examine the literature, he has asked some colleagues, and he 'came across' an article of a certain Dr Stekel.

Freud had one more answer for those who think that sexual abuse of children is too infrequent to be the cause of hysteria.

Lastly, the findings of my analysis are in a position to speak for themselves. In all eighteen cases (cases of pure hysteria and of hysteria combined with obsessions, and comprising six men and twelve women) I have, as I have said, come to learn of sexual experiences of this kind in childhood. I can divide my cases into three groups, according to the origin of the sexual stimulation. In the first group it is a question of assaults – of single, or at any rate isolated, instances of abuse, mostly practised on female children, by adults who were strangers, and who, incidentally, knew how to avoid inflicting gross, mechanical injury. In these assaults there was no question of the child's consent, and the first effect of the experience was preponderantly one of fright. The second group consists of the much more numerous cases in which some adult looking after the child – a nursery maid or governess or tutor, or, unhappily all too often, a close relative – has initiated the child into sexual intercourse and has maintained a regular love relationship with it – a love relationship, moreover, with its mental side developed – which has often lasted for years. The third group, finally, contains child-relationships proper – sexual relations between two children of different sexes, mostly a brother and sister, which are often prolonged beyond puberty and which have the most far-reaching consequences for the pair. (Freud 1896c: 207, 208)

This is the only place in the whole article where Freud wrote explicitly that in the early childhoods of all eighteen patients he had discovered sexual

⁴ This is the bibliographical reference Freud himself gave. It is wrong: 1896 should be 1895. This mistake has silently been corrected in the Standard Edition.

experiences. The words ‘as I have said’ suggest that he had already said it earlier, but that is not so. Earlier on he had written only that he had discovered sexual experiences of all eighteen patients during their puberty. Another remarkable aspect of this passage is that here, for the first time, Freud reported the identities of the people with whom his patients, as children, had had their sexual experiences. At least some of them seem to have been females (‘a nursery maid or governess’). We shall see, however, that other evidence of Freud’s seems inconsistent with the ‘assaulters’ having been female.

Assuming, as Freud did, that sexual ‘assaults’ in childhood occurred often, why, then, was hysteria not more common? Freud replied that, although the tubercle bacillus is the cause of tuberculosis, not everybody exposed to this bacillus falls ill. Freud also said:

the aetiological pretensions of the infantile scenes rest not only on the regularity of their appearance in the anamneses of hysterics, but, above all, on the evidence of there being associative and logical ties between those scenes and the hysterical symptoms – evidence which, if you were given the complete history of a case, would be as clear as daylight to you. (Freud 1896c: 210)

However, Freud did not present any complete case history, and we may wonder why not.

Freud supposed that sexual child abuse leads to hysteria only if the abuse has been frequent (Freud 1896c: 210), and that probably constitutional factors play a role. Further, for Freud, early sexual experiences lead to hysteria only if the memories of these experiences are repressed into the unconscious. That is why the existence of adults who remember having been sexually abused in childhood and are not hysterical does not contradict Freud’s thesis:

With our patients, those memories are never conscious (Freud 1896c: 211)

And:

The scenes must be present as *unconscious memories*; only so long as, and in so far as, they are unconscious are they able to create and maintain hysterical symptoms. (Freud 1896c: 211)

Why do some people and not other people remember the scenes? That – said Freud – ‘is a fresh problem, which we shall prudently avoid’. (Freud 1896c: 211)

★ ★ ★

Freud dealt with a third objection:

Our view then is that infantile sexual experiences are the fundamental precondition for hysteria, are, as it were, the *disposition* for it and that it is

they which create the hysterical symptoms, but that they do not do so immediately, but remain without effect to begin with and only exercise a pathogenic action later, when they have been aroused after puberty in the form of unconscious memories. If we maintain this view, we shall have to come to terms with the numerous observations which show that a hysterical illness may already make its appearance in childhood and before puberty. (Freud 1896c: 212)

This objection looks quite convincing. According to Freud hysteria is caused by memories that become pathogenic only after the beginning of puberty. It seems impossible to reconcile this theory with the fact that often hysteria can already be found before puberty. How did Freud answer this objection?

This difficulty, however, is cleared up as soon as we examine more closely the data gathered from analyses concerning the chronology of the infantile experiences. We then learn that in our severe cases the formation of hysterical symptoms begins – not in exceptional instances, but, rather, as a regular thing – at the age of eight, and that the sexual experiences which show no immediate effect invariably date further back, into the third or fourth, or even the second year of life. Since in no single instance does the chain of effective experiences break off at the age of eight, I must assume that this time of life, the period of growth in which the second dentition takes place, forms a boundary line for hysteria, after which the illness cannot be caused. (Freud 1896c: 212)

According to Freud, then, hysteria is caused by something which becomes active after puberty – even though he acknowledged that hysteria often starts long before puberty.

This answer also contained new information, as here Freud reported for the first time that his patients were between one and three years old when they were sexually abused. Until now Freud had spoken more vaguely of ‘infantile experiences’ or experiences in ‘early childhood’. Now, when meeting an objection to his theory, Freud felt forced to specify this period more precisely, because he had to postulate a long period between the sexual experiences and the onset of hysteria.

★ ★ ★

The fourth objection, Freud wrote, was that it might seem improbable that memories of experiences made his patients ill, while the experiences themselves had not caused any damage.

But we shall have to tell ourselves that this is a purely psychological problem. (Freud 1896c: 213)

and

that this problem may be allowed to remain unsolved for the time being,

without detracting from the value of the insight we have so far gained into the aetiology of hysterical phenomena. (Freud 1896c: 213, 214)

Some final remarks

In the fifth and last instalment of Freud's article, he wrote that not all hysterical symptoms go back to the earliest sexual experiences, but that some symptoms in every patient do. Those symptoms especially concern pain when urinating and defecating, intestinal disturbances, choking and vomiting, indigestion and disgust at food, which can be explained easily by the nature of the infantile experiences:

For the idea of these infantile sexual scenes is very repellent to the feelings of a sexually normal individual; they include all the abuses known to debauched and impotent persons, among whom the buccal cavity and the rectum are misused for sexual purposes. (Freud 1896c: 214)

Here, near the end of the article, Freud presented new information about the sexual events: in all cases 'debauched and impotent persons' used the buccal cavities and the rectums of the one-to-three year old children. If so, the culprits must have been exclusively or predominantly male. The word 'impotent' (*Impotenten* in German) can be used only for males – and presumably they were the ones who used the buccal cavities. It is also difficult to imagine the expression 'debauched persons' (*Wüstlingen* in German) being used for women. Yet earlier on, Freud had referred to nursery maids and governesses.

At the very end Freud said:

Prepared as I am to meet with contradiction and disbelief, I should like to say one thing more in support of my position. Whatever you may think about the conclusions I have come to, I must ask you not to regard them as the fruit of idle speculation. They are based on a laborious individual examination of patients which has in most cases taken up a hundred or more hours of work. What is even more important to me than the value you put on my results is the attention you give to the procedure I have employed. This procedure is new and difficult to handle, but it is nevertheless irreplaceable for scientific and therapeutic purposes. You will realize, I am sure, that one cannot properly deny the findings which follow from this modification of Breuer's procedure so long as one puts it aside and uses only the customary method of questioning patients. To do so would be like trying to refute the findings of histological technique by relying upon macroscopic examination. The new method of research gives wide access to a new element in the psychical field of events, namely, to processes of thought which have remained unconscious – which, to use Breuer's expression, are 'inadmissible to consciousness'. Thus it inspires us with the hope of a new and better understanding of all functional psychical disturbances. I cannot believe that psychiatry will long hold back from making use of this new pathway to knowledge. (Freud 1896c: 220, 221)

Thus Freud ended his article with a plea to use his new ‘pathway to knowledge’, his ‘modification of Breuer’s procedure’. However, he did not explain what this procedure consisted of.

Two years later he again suggested that no one could dispute his results because no one knew his method:

Basing myself on the ‘cathartic’ method introduced by Josef Breuer, I have in recent years almost completely worked out a therapeutic procedure which I propose to describe as ‘*psycho-analytic*’. I owe a great number of successes to it, and I hope I may be able further to increase its effectiveness considerably. The first accounts of the technique and scope of this method were given in *Studies on Hysteria*, written jointly with Breuer and published in 1895. Since then a good deal, as I think I may say, has been altered for the better. Whereas at that time we modestly declared that we could undertake only to remove the symptoms of hysteria, not to cure hysteria itself, this distinction has since come to seem to me without substance, so that there is a prospect of a genuine cure of hysteria and obsessions. It is therefore with very lively interest that I have read in the publications of colleagues that ‘in this case the ingenious procedure devised by Breuer and Freud has failed’, or that ‘the method has not performed what it seemed to promise’. This gave me something of the feelings of a man who reads his own obituary in the paper, but who is able to reassure himself by his better knowledge of the facts. For the method is so difficult that it has quite definitely to be learned; and I cannot recall that a single one of my critics has expressed a wish to learn it from me. Nor do I believe that, like me, they have occupied themselves with it intensely enough to have been able to discover it for themselves. The remarks in the *Studies on Hysteria* are totally inadequate to enable a reader to master the technique, nor are they in any way intended to give any such complete instruction. (Freud 1898: 282)

This is quite clear: nobody could dispute Freud’s results, because nobody knew his method of obtaining them.

THE SEDUCTION THEORY IN FREUD’S LATER PUBLICATIONS (1898–1925)

However unclear or unconvincing the seduction theory may be, trying to prove it wrong makes no sense: it is impossible to prove that any patient does *not* suffer from unconscious memories about very early sexual experiences. Judging the validity of the theory was also quite impossible because Freud had reached his results by using a method which no one knew.

1898: Directing attention

In an 1898 article, Freud mentioned his three articles of 1896 in which he had presented his seduction theory, and then wrote:

A full presentation [of the 1896 ideas] is, however, still wanting, mainly

because, in endeavouring to throw light on what is recognized as the actual state of affairs, we come upon ever fresh problems for the solution of which the necessary preliminary work has not been done. It does not seem to me at all premature, however, to attempt to direct the attention of medical practitioners to what I believe to be the facts so that they may convince themselves of the truth of my assertions and at the same time of the benefits they may derive in their practice from a knowledge of them. (Freud 1898: 263)

Freud did not say what these 'fresh problems' were; but whatever their nature, it seems clear that in 1898 Freud still publicly adhered to his seduction theory.

1905: Illustration, addition and correction

Between 1898 and 1905 Freud, in his publications, did not deal explicitly with his 1896 theory. In 1905 Freud mentioned his earlier theory in several publications, the first being a case history of a hysterical patient known pseudonymously as Dora. The case history began:

In 1895 and 1896 I put forward certain views upon the pathogenesis of hysterical symptoms and upon the mental processes occurring in hysteria. Since that time several years have passed. In now proposing, therefore, to substantiate those views by giving a detailed report of the history of a case and its treatment, [. . .] (Freud 1905a: 7)

Freud's 1896 views were that the cause of hysteria is found in unconscious memories of very early sexual traumas. In this 1905 case history there were indeed some sexual traumas. However, the earliest reported one took place not when – as the 1896 theory had alleged – the patient called Dora was between one and three years old, but when she was fourteen. The traumatic experience was that an adult male unexpectedly tried to kiss her: the man

suddenly clasped the girl to him and pressed a kiss upon her lips. This was surely just the situation to call up a distinct feeling of sexual excitement in a girl of fourteen who had never before been approached. But Dora had at that moment a violent feeling of disgust, tore herself free from the man, and hurried past him to the staircase and from there to the street door. [. . .] In this scene [. . .] the behaviour of this child of fourteen was already entirely and completely hysterical. I should without question consider a person hysterical in whom an occasion for sexual excitement elicited feelings that were preponderantly or exclusively unpleasurable; and I should do so whether or no the person were capable of producing somatic symptoms. The elucidation of the mechanism of this *reversal of affect* is one of the most important and at the same time one of the most difficult problems in the psychology of the neuroses. In my own judgement I am still some way from having achieved this end; and I may add that within the limits of the present paper I shall be able to bring forward only a part of such knowledge on the subject as I do possess. (Freud 1905a: 28, 29)

We quote this not in order to show that Freud had curious opinions about interaction between males and females: that 'a girl of fourteen who has never before been approached' does not like being kissed suddenly by an adult male was for Freud sufficient proof that she was suffering from hysteria. We quote this because Freud wrote that, when his patient was fourteen, her behaviour had already been 'completely hysterical', and that he was still some way from explaining this hysterical 'reversal of affect'. Yet, in 1896, in his seduction theory Freud had been perfectly capable of explaining such a 'reversal': according to the seduction theory such hysterical reversal is caused by the unconscious memory of a very early sexual trauma.

In 1905 Freud also mentioned his seduction theory of 1896 in a booklet called *Three Essays on Sexuality*. In a section on 'seduction' of children by adults or by other children Freud wrote:

I cannot admit that in my paper on 'The Aetiology of Hysteria' [1896c] I exaggerated the frequency or importance of that influence, though I did not then know that persons who remain normal may have had the same experiences in their childhood, and though I consequently overrated the importance of seduction in comparison with the factors of sexual constitution and development. (Freud 1905b: 190)

The seduction theory, said Freud, now must be revised: in 1896 he had not known that there are people who have had similar childhood experiences and have not become hysterical. This view seems perfectly plausible, until it is compared with the content of Freud's 1896 paper, 'The Aetiology of Hysteria', where he had written

that it is easy, by making a few enquiries, to find people who remember scenes of sexual seduction and sexual abuse in their childhood years, and yet who have never been hysterical. (Freud 1896c: 207)

So in 1896 Freud had known of people who were sexually abused in their childhood and did not become hysterical. And, in 1896, he had explained convincingly that it is an error to think that this fact contradicts the seduction theory:

We have heard and have acknowledged that there are numerous people who have a very clear recollection of infantile sexual experiences and who nevertheless do not suffer from hysteria. This objection has no weight; but it provides an occasion for making a valuable comment. According to our understanding of the neurosis, people of this kind *ought* not to be hysterical at all, or at any rate, not hysterical as a result of the scenes which they consciously remember. With our patients, those memories are never conscious; but we cure them of their hysteria by transforming their unconscious memories of the infantile scenes into conscious ones. (Freud 1896c: 211)

However, now, in 1905, Freud wrote that in 1896 he had not known of people

who had had infantile sexual experiences and did not suffer from hysteria, and that this new information would have forced him to revise his seduction theory. Why did Freud in 1905 want to give the impression that nine years ago he had not known something which he certainly had known?

In 1905 Freud wrote still one more text on the seduction theory. In that text, published in 1906, Freud summarized the theory, and wrote:

Though even to-day I do not consider these assertions incorrect, it is not to be wondered at that, in the course of ten years of continuous effort at reaching an understanding of these phenomena, I have made a considerable step forward from the views I then held, and now believe that I am in a position, on the basis of deeper experience, to correct the insufficiencies, the displacements and the misunderstandings under which my theory then laboured. At that time my material was still scanty, and it happened by chance to include a disproportionately large number of cases in which sexual seduction by an adult or by older children played the chief part in the history of the patient's childhood. I thus over-estimated the frequency of such events (though in other respects they were not open to doubt). (Freud 1906:274)

On its own, this passage sounds perfectly plausible; however, it contradicts the passage just quoted from the *Three Essays on Sexuality* where Freud wrote that he can *not* admit that in 1896 he had overestimated the frequency of 'seduction' in childhood among hysterics.

Freud mentioned another reason why in 1896 he had overestimated the frequency of sexual child abuse:

I was at that period unable to distinguish with certainty between falsifications made by hysterics in their memories of childhood and traces of real events. Since then I have learned to explain a number of fantasies of seduction as attempts at fending off memories of the subject's *own* sexual activity (infantile masturbation). (Freud 1906: 274)

Freud implied here that he had somehow overcome his previous inability to distinguish between falsified memories and real events. However, he did not explain how he had managed this. In 1896 he had considered the possibility that 'patients tell the physician things which they have deliberately invented or have imagined and that he accepts those things as true' – and Freud had rejected that possibility and given arguments for that rejection. Now, in 1905, he did not explain what had been wrong with those arguments, and did not even mention them.

Freud also gave a third reason for having to revise his seduction theory:

Further information now became available relating to people who had remained normal; and this led to the unexpected finding that the sexual history of *their* childhood did not necessarily differ in essentials from that of neurotics, and, in particular, that the part played by seduction was the same in both cases. As a consequence, accidental influences receded still further into the background as compared with 'repression' (as I now began to say

instead of 'defence'). Thus it was no longer a question of what sexual experiences a particular individual had had in his childhood, but rather of his reaction to those experiences – of whether he had reacted to them by 'repression' or not. (Freud 1906: 276, 277)

This argument was quite similar to the earlier argument in the *Three Essays*. Here again Freud wanted us to believe that in 1896 he had not known that there are people who have been sexually abused in their childhood and nevertheless have remained healthy. He also wanted us to believe that in 1896 he had thought that people became hysterical solely because as children they had been sexually abused, and that only later had he discovered that what matters is whether these experiences have been repressed from conscious memory or not. In fact, as we have seen, in 1896 Freud had stated emphatically that people became hysterical only if they have repressed these memories.

This text of Freud was published in the fourth edition of a book called *Sexualleben und Nervenleiden* (Sexual Life and Nervous Disease) written by the German psychiatrist L. Löwenfeld. The first edition of the book had come out before Freud had published his seduction theory. In the second edition of 1899 and the third of 1903 Löwenfeld had commented extensively on Freud's theory of 1896:

Until now Freud with his ideas has remained rather a voice in the wilderness; partly there has just remained silence about his ideas, and partly they have just simply been rejected [. . .] However, here not only our subject matter but also the importance which Freud's research should have for every serious researcher force us to present at least a brief discussion of his theory. (Löwenfeld 1899: 192, 193)

After summarizing the seduction theory, Löwenfeld wrote:

Freud states that he could prove the connection between the symptoms and an infantile sexual scene with eighteen cases of hysteria which he analysed with the help of a method which cannot be described here. However, if we see what this proof actually looks like according to Freud's own report, we cannot attribute any value to it. Freud writes: 'Before they come for analysis the patients know nothing about these scenes. They are indignant as a rule if we warn them that such scenes are going to emerge. Only the strongest compulsion of the treatment can induce them to embark on a reproduction of them. While they are recalling these infantile experiences to consciousness, they suffer under the most violent sensations, of which they are ashamed and which they try to conceal; and, even after they have gone through them once more in such a convincing manner, they still attempt to withhold belief from them, by emphasizing the fact that, unlike what happens in the case of other forgotten material, they have no feeling of remembering the scenes.' These remarks show two things: 1. The patients were subjected to a suggestive influence coming from the person who analysed them, by which the rise of the mentioned scenes was brought quite close to their imagination. 2. These fantasy pictures that had arisen under the influence of the analysis were

definitively denied recognition as memories of real events. I also have a direct experience to support this second conclusion. By chance, one of the patients with whom Freud used the analytic method came under my observation. The patient told me with certainty that the infantile sexual scene which analysis had apparently uncovered was pure fantasy and had never really happened to him. It is difficult to understand how a researcher like Freud, who normally is very critical, despite such remarks, still could maintain toward his patients that the pictures that arose in their minds were memories of real events. However, it is even still more difficult to understand that Freud thought that he could consider this assumption to be completely proven in each single case of hysteria. (Löwenfeld 1899: 195, 196)⁵

This passage, which can be found in the second (1899) and third (1903) edition of Löwenfeld's book, was omitted from the fourth edition (1906) and was replaced by the text of Freud which was just discussed. Löwenfeld's account of the patient is wholly consistent with Freud's remark that even after his hysterical patients 'have gone through' the scenes in treatment, 'they have no feeling of remembering' them.

Let us now summarize the contradictions among Freud's various published statements of 1905 about his earlier theory. In the Dora case Freud said he wished to substantiate his 1896 theory, although the essence of that theory was missing in the Dora case history. In the *Three Essays* and in the Löwenfeld book Freud wrote that he had now revised his earlier theory, because in 1896 he would not yet have known of people who have been sexually abused in their childhood and have not become hysterical – which is not so: in 1896 Freud had known this perfectly well, and had explained why this fact does not contradict the seduction theory. In the *Three Essays* Freud wrote that in 1896 he had not over-estimated the frequency of sexual child abuse among his hysterical patients; in the Löwenfeld book Freud wrote that he had over-estimated it. In the Löwenfeld book Freud suggested that in 1896 he had believed that people become hysterical because in childhood they were sexually abused, and that only after 1896 he had learnt that what matters is whether or not they had repressed these experiences. Yet in reality in 1896 Freud had argued that people do not become hysterical as a result of those experiences alone but as a result of repressing the memories of them.

1914: An almost fatal mistake

The confusion becomes even greater in Freud's writings about his seduction theory after 1905. In 1914 Freud characterized his earlier theory as

a mistaken idea [. . .] which might have been almost fatal to the young

⁵ This interesting passage has been discovered by Jeffrey Masson. Masson did not use it in his book on the seduction theory, *The Assault on Truth*, presumably because he wanted us to believe that the patients in 1896 had told Freud spontaneously that they had been sexually abused. However, Masson deserves credit for publishing this discovery at all, which he did in a footnote to the Freud-Fliess correspondence (Masson 1985: 413). Unfortunately in the German edition this footnote has been removed (Freud 1986: 452).

science. Influenced by Charcot's view of the traumatic origin of hysteria, one was readily inclined to accept as true and aetiologically significant the statements made by patients in which they ascribed their symptoms to passive sexual experiences in the first years of childhood – to put it bluntly, to seduction. When this aetiology broke down under the weight of its own improbability and contradiction in definitely ascertainable circumstances, the result at first was helpless bewilderment. Analysis had led back to these infantile sexual traumas by the right path, and yet they were not true. (Freud 1914: 17)

Here the general tone was quite different from 1905. It looks as if between 1905 and 1914, Freud must have discovered that his 1896 patients had not had a real history of sexual abuse at all! How could Freud have discovered this after all these years? Freud now mentioned two reasons for abandoning the seduction theory. Firstly: the theory broke down under the weight of its own improbability. This does not sound convincing: also in 1896 Freud must have realized that his seduction theory seemed improbable. Secondly: the theory was contradicted 'in definitely ascertainable circumstances'. It is difficult to imagine what that could mean. In 1896 Freud had thought that people become hysterical because they repress memories of having been sexually abused in early childhood by an unknown adult, by persons such as a nursery maid, a governess, or by another child. How could it be possible to ascertain definitively that this is not so? According to the seduction theory, the hysterical patients do not remember these experiences. Had Freud asked the nursery maids or governesses? As if their denials would prove anything.⁶

1925: Freud as naïve and fathers as seducers

Many people have got a general idea – either directly or indirectly – of what the seduction theory actually is from two Freud texts: the just quoted passage of 1914, and a passage in Freud's *Autobiographical Study* of 1925:

Before going further into the question of infantile sexuality I must mention an error into which I fell for a while and which might well have had fatal consequences for the whole of my work. Under the influence of the technical procedure which I used at that time, the majority of my patients reproduced

⁶ Frank Cioffi tried to imagine how Freud would have organized his 'definitely ascertainable circumstances':

'Your daughter, Sir, has alluded to some striking peculiarities in your sexual character. I wonder whether, in the interests of science, you would care to authenticate, and perhaps enlarge on them.' It can't often have happened this way. And, in any case, what would a denial prove? (Cioffi 1984: 743)

Cioffi assumed here that, according to the seduction theory, it is the father who abuses his own daughter. However, in 1914 there was not yet any indication that the culprit in the seduction theory was the father.

from their childhood scenes in which they were sexually seduced by some grown-up person. With female patients the part of seducer was almost always assigned to their father. I believed these stories, and consequently supposed that I had discovered the roots of the subsequent neurosis in these experiences of sexual seduction in childhood. My confidence was strengthened by a few cases in which relations of this kind with a father, uncle, or elder brother had continued up to an age at which memory was to be trusted. If the reader feels inclined to shake his head at my credulity, I cannot altogether blame him; though I may plead that this was at a time when I was intentionally keeping my critical faculty in abeyance so as to preserve an unprejudiced and receptive attitude towards the many novelties which were coming to my notice every day. When, however, I was at last obliged to recognize that these scenes of seduction had never taken place, and that they were only fantasies which my patients had made up or which I myself had perhaps forced on them, I was for some time completely at a loss. [. . .] I do not believe even now that I forced the seduction-fantasies on my patients, that I 'suggested' them. [. . .] Moreover, seduction during childhood retained a certain share, though a humbler one, in the aetiology of neuroses. But the seducers turned out as a rule to have been older children. (Freud 1925: 33–35)

Here Freud wrote that in 1896 he had believed his patients' stories of sexual abuse in childhood. This was – Freud now admitted – naïve, but at that time he had tried to be unprejudiced and receptive towards the many novelties that came to him. The distortion which Freud made here of his earlier theory had already been somewhat apparent in 1914, when he had reported that he had been inclined to accept as true 'statements made' by his patients. It is this distortion which has dominated most people's view of what the seduction theory actually is about.

The 1905, 1914 and 1925 remarks of Freud about his earlier seduction theory clearly are attempts at explaining why he had been forced to revise (1905) or abandon (since 1914) his earlier theory. This strategy of Freud has been quite successful: we do not know anyone who before 1972 noticed that these reasons could not be true, and since then only very few people have mentioned this.⁷

In 1925 Freud introduced a new element into the seduction theory: that with the female patients the alleged, or fantasied, seducer nearly always was the father. Neither in his initial articles on the seduction theory, nor in his later remarks of 1905 or 1914, did Freud say that in 1896 he had believed that the fathers were especially implicated. In 1896 he had mentioned three categories of child abusers: (1) adults who did not know the child; (2) adults who took care of the child, and (3) other children. In the second category Freud had mentioned

⁷ Frank Cioffi (1972 and 1974) was the first. By far the best survey has been given by Vetter (1988). The London psychotherapist Anthony Stadlen was another one, and we have especially profited from talks with him.

governesses, nursery maids, tutors, and – as a last possibility – ‘unhappily all too often, a close relative’. ‘A close relative’ can be a father, but Freud had not specifically mentioned fathers. In 1905 and 1914 Freud had written nothing about the identity of the ‘seducers’. Yet, in 1925, he wrote as if in 1896 he had thought that the abusers of nearly all his female patients had been their own fathers.

The introduction in 1925 of the fathers as the main category did not come as a complete surprise. Already in 1916 Freud had written:

Fantasies of being seduced are of particular interest, because so often they are not fantasies but real memories. Fortunately, however, they are nevertheless not real as often as seemed at first to be shown by the findings of analysis. Seduction by an older child or by one of the same age is even more frequent than by an adult; and if in the case of girls who produce such an event in the story of their childhood their father figures fairly regularly as the seducer, there can be no doubt either of the imaginary nature of the accusation or of the motive that has led to it. (Freud 1916: 370)⁸

So here in 1916 Freud wrote that ‘fairly regularly’ girls tell stories, which he had once believed, that the father is the seducer.⁹

A LETTER OF 1897, PUBLISHED IN 1950

In 1950 new information came to light that can elucidate why Freud gave up the seduction theory. A selection of letters was published which Freud had written between 1887 and 1904 to his best friend Wilhelm Fliess. Freud had never intended these letters for publication. When, in the 1930s, he discovered that they had been preserved, he wrote worriedly:

‘I do not want any of them to become known to so-called posterity’ (quoted in Masson 1985: 7).

Freud’s wish has not been respected; in 1950 a selection of the letters was published (in German), edited by three prominent psychoanalysts: Marie Bonaparte, Anna Freud and Ernst Kris.

In a letter to Fliess in September 1897, Freud said that he no longer believed in his theories on the neuroses, including his seduction theory, and explained why. We translate the beginning of that letter as it was published in the German edition of 1950:

Dear Wilhelm,
Here I am again, since yesterday morning, refreshed, cheerful, impoverished,

⁸ This does not mean that Freud completely denied the existence of incest. In the same passage Freud wrote: ‘You must not suppose, however, that sexual abuse of a child by its nearest male relatives belongs entirely to the realm of fantasy.’ (Freud 1916: 370)

⁹ This overview of Freud’s remarks on the seduction theory in his later publications is not exhaustive. For a more complete overview, see Vetter 1988: 25–43. See also Masson 1984: 123–129.

at present without work, and having settled in again, I am writing to you first.

And now I want to confide in you immediately the great secret that has been slowly dawning on me in the last few months. I no longer believe in my neurotica. This is probably not intelligible without an explanation; after all, you yourself found credible what I was able to tell you. So I will begin historically where the motives for disbelief came from. The continual disappointment in my efforts to bring my analysis to a real conclusion; the running away of people who for a period of time had been most gripped; the absence of the complete successes on which I had counted; the possibility of explaining to myself the partial successes in other ways, in the usual fashion – this was the first group. Then the surprise that in all cases, the father . . . had to be accused of being perverse – the realization of the unexpected frequency of hysteria, with precisely the same conditions prevailing in each, whereas surely such widespread perversions against children are not very probable. (The perversion would have to be immeasurably more frequent than the hysteria because the illness, after all, occurs only where there has been an accumulation of events and there is a contributory factor that weakens the defence.) Then, third, the certain insight that there are no indications of reality in the unconscious, so that one cannot distinguish between truth and fiction that has been cathected with affect. (Accordingly, there would remain the solution that the sexual fantasy invariably seizes upon the theme of the parents.) Fourth, the consideration that in the most deep-reaching psychosis the unconscious memory does not break through, so that the secret of childhood experiences is not disclosed even in the most confused delirium. If one thus sees that the unconscious never overcomes the resistance of the conscious, the expectation that in treatment the opposite is bound to happen, to the point where the unconscious is completely tamed by the conscious, also diminishes.

I was so far influenced that I was ready to give up two things: the complete resolution of a neurosis and the certain knowledge of its aetiology in childhood. Now I have no idea of where I stand because I have not succeeded in gaining a theoretical understanding of repression and its interplay of forces. It seems once again arguable that only later experiences give the impetus to fantasies, which hark back to childhood, and with this the factor of a hereditary disposition regains a sphere of influence from which I had made it my task to dislodge it – in the interest of illuminating neurosis.

If I were depressed, confused, exhausted, such doubts would surely have to be interpreted as signs of weakness. Since I am in an opposite state, I must recognize them as the result of honest and vigorous intellectual work and must be proud that after going so deep I am still capable of such criticism. Can it be that this doubt merely represents an episode in the advance toward further insight?

It is strange, too, that no feeling of shame appeared – for which, after all, there could well be occasion. Of course I shall not tell it in Dan, nor speak of it in Askelon, in the land of the Philistines, but in your eyes and my own, I have more the feeling of a victory than a defeat (which is surely not right). (Freud 1950: 229–231)

It is immediately clear that this letter offered a wealth of information that can help to explain why Freud had given up his seduction theory. Freud wrote that he would not tell in Dan and Askelon, 'in the land of the Philistines', that he no longer believed in his seduction theory. This is remarkable. One and a half years earlier, Freud had characterized his theory as a 'caput Nili'; if he now thought that this source of the Nile was in fact a mirage, one might expect him to let the world know.

In his letter Freud presented four groups of motives for no longer believing in his seduction theory. All these motives are different from the reasons he mentioned in his later publications. We will discuss these four groups of motives in reverse order.

Deep-reaching psychosis

The last motive Freud mentioned is

the consideration that in the most deep-reaching psychosis the unconscious memory does not break through, so that the secret of childhood experiences is not disclosed even in the most confused delirium. If one thus sees that the unconscious never overcomes the resistance of the conscious, the expectation that in treatment the opposite is bound to happen, to the point where the unconscious is completely tamed by the conscious, also diminishes.

Given that the unconscious memories, so to speak, did not 'break through' even in the 'most confused delirium', the possibility is strong that the childhood experiences in question had never really occurred.

The certain insight

As the last but one motive Freud mentioned

the certain insight that there are no indications of reality in the unconscious, so that one cannot distinguish between truth and fiction that has been cathected with affect.

According to the seduction theory, the scenes reproduced during treatment were the products of the patient's unconscious, and in 1896 Freud had presented seven answers to meet the objection that these scenes were products of the patients' fantasies (or of Freud's suggestion). In the 1897 letter, Freud now wrote that information coming from the unconscious does not indicate whether it refers to real events or whether it is only 'fiction that has been cathected with affect'. Nine years later, in 1906, Freud wrote that in 1896 he had been

unable to distinguish with certainty between falsifications made by hysterics in their memories of childhood and traces of real events. Since then I have learned to explain a number of fantasies of seduction as attempts at fending off memories of the subject's *own* sexual activity (infantile masturbation). (Freud 1906: 274)

Freud did not say how now in 1906 he had learnt to make this distinction. In summary: in 1896 Freud presented seven arguments explaining how he could distinguish between fantasies and real memories; in 1897 he wrote to Fliess that he possessed the 'certain insight' that this distinction cannot be made, without explaining why this insight was so certain or what has been wrong with his previous arguments, and in 1906 he had again a different insight, namely that now he was able to make this distinction but that in 1896 he had not yet been able to make it.

Without case material, it is impossible to choose a preference among Freud's various 'insights'. Suppose the patients, when reproducing the sexual scenes, had presented as harmless events which would have horrified them had they understood them, or suppose Freud had presented a case history where the sexual scenes had fit in like the last pieces of a jigsaw puzzle. But Freud did not give such evidence.

Perverse fathers

In the 1897 letter, Freud mentioned a second group of motives for no longer believing in his seduction theory:

the surprise that in all cases, the father . . . had to be accused of being perverse – the realization of the unexpected frequency of hysteria, with precisely the same conditions prevailing in each, whereas surely such widespread perversions against children are not very probable. (The perversion would have to be immeasurably more frequent than the hysteria because the illness, after all, occurs only where there has been an accumulation of events and there is a contributory factor that weakens the defence.)

The three dots indicate words that in the published version of 1950 were omitted. Despite the omitted words, this passage contains much new information, in particular that in all cases the father 'had to be accused of being' perverse, and that hysteria is more frequent than Freud had expected, although so much perversion against children is improbable. In the course of 1896 and 1897 Freud had begun to detect more and more hysteria in people around him. On 11 February 1897 he wrote to Fliess that he had now discovered hysteria in his own brother and in several of his sisters, and on 14 August 1897 he mentioned to Fliess his own 'little hysteria'. In the previous section an example was given of how a few years later Freud detected the existence of hysteria: when fourteen years old Dora did not appreciate the unexpected and passionate kisses of an adult male, this was sufficient proof for Freud that she was hysterical. So the 'unexpected frequency of hysteria' may have resulted from Freud's deployment of the concept 'hysteria'.

In the spring of 1896 (the article) Freud had referred to all kinds of abusers of young children. In the letters to Fliess before December 1896 no remarks about the identity of the culprits can be found. In a letter to Fliess of 6 December 1896 Freud mentioned a father as the culprit. On 3 January 1897 he exclaimed triumphantly: 'Habemus papam' when he reported about one more case of a father who had abused his daughter. So maybe in the course of 1896/1897 Freud changed his mind about the identity of the child abusers; maybe in the fall of 1897 Freud had come to

the conclusion that he had made mistakes when identifying the perpetrators with his eighteen cases of 1896. However, this is not very probable. How could Freud have discovered afterwards that his previous identifications had been wrong?

Another explanation which we think more probable is that in the spring of 1896 Freud was already convinced that it was especially fathers who sexually abused their little daughters. In the 1896 article Freud had mentioned several kinds of female perpetrators such as nursery maids and governesses, but further on he had characterized them as 'debauched and impotent persons' using the buccal cavity and rectum for sexual purposes – which implies male offenders. From about 1920 onward Freud started to write that in 1896 he had thought that nearly all his female hysterics had been abused by their fathers. In 1895 he had described the case histories of two female patients who had been sexually abused by an uncle; nearly thirty years later Freud added a footnote in which he revealed that in both cases the seducer had been not the uncle but the father (Freud 1924: 121 and 164). If already in 1896 Freud thought that the sexual abusers were the fathers, why at first did not he say so? Maybe Freud did not wish to offend his audience by alleging that all those men whose offspring suffered from hysteria had sexually abused them in early childhood.

This second group of motives contains three intriguing dots. In 1966 what had been missing in the 1950 edition was revealed. Four words – 'not excluding my own' – had been left out, so the complete passage reads:

that in all cases, the father, not excluding my own, had to be accused of being perverse. (Freud 1966: 259)

These four previously omitted words have resulted in hundreds of pages of commentary. Obviously one motive for no longer believing in the seduction theory was that Freud thought it improbable that his own father had abused his children. Freud's father had died a year before the letter was written. Several authors (Balmory, Krüll, Schiphorst) have argued that Freud abandoned his seduction theory because he could not bear making such a monstrous accusation against his own father. According to these authors, Freud's abandonment of the seduction theory must be explained not by rational motives, but by affective ones: unconscious feelings for his father. We will show that such deep psychological explanations are superfluous and that Freud gave up his seduction theory for motives of which he was completely conscious.

The absence of success

The first group of motives mentioned by Freud for no longer believing in his seduction theory were:

The continual disappointment in my efforts to bring my analysis to a real conclusion; the running away of people who for a period of time had been most gripped; the absence of the complete successes on which I had counted;

the possibility of explaining to myself the partial successes in other ways, in the usual fashion – this was the first group.

The absence of complete successes seems to contradict Freud's published statements one-and-a-half years earlier, even though, in Freud's 1896 article, he had not alleged universal success: he had said only that with all eighteen patients he had had therapeutic success 'where the circumstances allowed', without specifying those 'circumstances'. Freud's 'continual disappointment' in his efforts to bring his 'analysis to a real conclusion' seemed to refer to his self-analysis. A few months earlier, he had started to try to analyse himself. We already mentioned a letter to Fliess of 14 August 1897 in which Freud wrote that he had now discovered hysteria in himself. It appears that by not managing to bring his analysis to a real conclusion he meant that he had not discovered how he as a little child had been sexually abused, presumably by his father, after which his hysterical symptoms would have disappeared.

Freud's official biographer Ernest Jones wrote about the abandonment of the seduction theory that

it is very possible that the decisive factor had been his own self-analysis (Jones 1953: 266).

This seems to supply the first motive for Freud abandoning his seduction theory: he could not find sufficient confirmation for the theory with his most important patient: himself. Freud did not present any information about how he might have performed this test on his own person, and nowhere else did he refer to his failure to conclude his self-analysis. Freud put his inability to conclude his own analysis together with three other motives, none of which are about himself but are about his patients: they ran away, they were not really cured, and the partial therapeutic successes could be explained differently. All three motives belong together and imply something like disappointment with his patients. Why did Freud put the problems he had with himself among this group of motives? We saw that Jones wrote that probably Freud's own analysis was the main factor for giving up the seduction theory. However, in the same paragraph Jones wrote that Freud in his 1897 letter had mentioned

four reasons for his growing doubts. First his numerous disappointments in not being able to bring his analyses to a proper completion (Jones 1953: 266).

Here it looks as if Jones made a mistake: as the first reason he did not mention Freud's inability to complete his own analysis, but to complete 'his analyses' – plural, which therefore must mean not his own analysis but the analyses of his patients! At first sight one might think that this is a slip of the pen or a printing error – 'analyses' instead of 'analysis' – but curiously enough the same alteration of one character can be found in the English translation of Freud's letters to Fliess as they were published in 1954, one year after Jones's book. Here the first motive has been translated as:

the continual disappointment of my attempts to bring my analyses to a real conclusion (Freud 1954: 215).

Is this only sloppiness?

THE LETTER OF 1897 AS IT WAS PUBLISHED IN 1984

A new transcription: not *'meine'* but *'eine'*

In the previous section we described how Freud in his 1897 letter explained why he no longer believed in his seduction theory and how the very first motive he mentioned was his continual disappointment in his efforts to bring his own analysis to a real conclusion. At least this is what can be read in the edition of 1950. However, this is not what Freud had actually written. In fact nowhere in his letter had Freud mentioned his own analysis: he did not write 'my analysis' (German: *'meine Analyse'*) at all. We saw how Jones wrote that the first reason was that Freud could not bring 'his analyses' (plural) to a real conclusion and, according to the 1954 English translation, Freud had written about not being able to bring 'my analyses' to a real conclusion, so one might be inclined to think that in reality Freud had not written 'my analysis' (German: *'meine Analyse'*), but 'my analyses' (German: *'meine Analysen'*). However, this also is not what can be read in the original letter. The first motive mentioned by Freud in his letter was:

Die fortgesetzten Enttäusschungen bei den Versuchen, *eine* [our italics] Analyse zum wirklichen Abschluss zu bringen. (Freud 1986: 283)

This new corrected transcription was used for the first time in 1984 by Jeffrey Masson in his book *The Assault on Truth*, where he translated it as:

The continual disappointment in my efforts to bring any analysis to a real conclusion. (Masson 1984: 108)

A year later the complete edition of Freud's letters to Fliess, edited by Masson, was published; here the first motive is translated as:

The continual disappointment in my efforts to bring a single analysis to a real conclusion. (Masson 1985: 264)

A year after that, the complete Freud-Fliess correspondence was published in German. Officially, this edition too was edited by Masson; in fact this edition was by people from the psychoanalytic establishment. In this new German edition too the transcription of this first motive was corrected: not *'meine Analyse'* (my analysis) but *'eine Analyse'* (any analysis, a single analysis), and the correction of the transcription was indicated by two tiny brackets. Masson did explicitly mention the correction of the transcription in a footnote both in his 1984 book and in his 1985 edition; however, he did not draw attention to the relevance of this new transcription. Does it have any relevance?

The new transcription showed that Freud's very first reason for losing his

belief in his seduction theory was his disappointment in not being able to bring any analysis to a real conclusion. So, when Freud wrote this, in September 1897, he had not been able to finish a single analysis. One-and-a-half years before, when presenting his seduction theory, Freud had alleged that his theory was based on eighteen completed analyses. Yet nothing indicates that in those eighteen months Freud's definition of a concluded analysis had changed.

All this is in tune with other behaviour of Freud. When in the eighties Freud had presented cocaine as a medicine against morphine addiction, he had always declared that he knew a case where that therapy had been a success. Yet he had known perfectly well that the case (Ernst Fleischl) had been a catastrophe. With Anna O. Freud alleged that her treatment ended with a therapeutic success, although Freud knew that in reality the treatment had come to an end because the patient had had to be put into a mental asylum. Here, with the seduction theory, the same pattern returned: again Freud argued for the credibility of a new theory by alleging successes which in reality were not there. With Ernst Fleischl and Anna O. Freud wrote that the therapy had been a success despite knowing that this was not true; here, with the seduction theory, it was not so much a matter of lying, but more, we suppose, of boasting, of announcing a result as already having been reached which he hoped to reach soon. In 1896 Freud firmly believed in the truth of the seduction theory, so firmly that he was sure of soon reaching the results that the theory predicted, and thus believed it was safe and legitimate to announce these results. This means that after publishing his seduction theory he must have felt strong pressure to reach these results. However, when trying to do that, he was disappointed again and again.

In July 1896, a month after the publication of the seduction theory, Freud wrote to Fliess:

I am frantically trying to 'finish' several people. (Masson 1985: 194)

In December he wrote:

so far not a single case is finished; I feel I am still missing an essential piece somewhere. As long as no case has been clarified and seen through to the end, I do not feel sure and I cannot be content. (Masson 1985: 218)

In January 1897 Freud proposed to Fliess a meeting at Easter, and added:

Perhaps by then I shall have carried one case to completion. (Masson 1985: 220)

In February Freud wrote about the plans for this meeting:

By then perhaps, one case will be entirely finished. Until this has been accomplished, there really can be no certainty. (Masson 1985: 230)

And in March Freud wrote:

I have not yet finished a single case. (Masson 1985: 232)

These repeated efforts explain why in the first motive in his letter of September

1897 Freud put special emphasis on his 'continual disappointment in my efforts' to bring any analysis to a real conclusion.

In the old transcription the very first motive had not fitted in well with the other three in the first group of motives; now these four motives clearly all belong into the same category: Freud wrote that he was continually disappointed when he tried to bring any analysis to a real conclusion, his most promising patients ran away, he did not produce real therapeutic successes, and the partial successes could be explained differently.

Under these circumstances it became more and more difficult for Freud to retain his belief in a theory that had to bring results which he could not reach. On the other hand it was difficult for Freud to give up his theory, not only because he had already published it, but also because he had alleged successes that would have been reached with the help of that theory. In the autumn of 1897 he returned from a long holiday to Vienna and wrote to Fliess that, after one-and-a-half years of trying in vain, he no longer believed in his theory. One can imagine that such a decision brought him relief.

It is evident now why Freud could not openly declare that he no longer believed in his seduction theory. At first sight it looked strange when Freud in his 1897 letter wrote about his new insights:

Of course I shall not tell it in Dan, nor speak of it in Askelon, in the land of the Philistines.

However, Freud could not say aloud that he had given up his seduction theory because this would mean admitting that he had boasted of successes he had not had. Instead, Freud kept silent for eight years. In 1905 he let his readers believe that he now had revised his theory somewhat, while only in 1914 did he start to write publicly that the theory had been a major mistake.

In his 1896 article Freud had written that the correctness of his seduction theory would be 'as clear as daylight' if he would present a complete case history, but that such a complete case history was missing for several reasons, of which he could mention one: that it would take too much time. It is now clear that he did not present a complete case history because he did not have any.

This reconstruction of the events around the seduction theory – Freud presented a new theory in which he believed so firmly that he felt justified to allege successes which he did not have – helps to explain the curious composition of his 1896 article. We have seen that the article had two parts. In the first part Freud reported how Breuer's trauma theory did not work, which was the reason to radicalize the theory – the traumas must have been earlier and more serious – and how along those lines Freud gradually, after more and more radicalization, at last reached the revolutionary idea of sexual abuse at a very early age. Immediately after this first part began the second part, which consisted of meeting objections that could be raised against this revolutionary idea. The emphasis of the article was not on empirical evidence that might support the theory, but on Freud's disappointment with theories in which he had believed

earlier and on his meeting objections. This composition reflects the true course of the events: Freud thought that his new theory had to be true, but had insufficient empirical evidence to confirm it. There is one more thing which can now be understood better. According to the psychoanalytic view the letter of September 1897 was a turning point in the development of Freud's thinking: Freud gave up the seduction theory, and thus created the possibility of developing his correct psychoanalytic theories. However, Masson has stressed that during the months following this letter of September 1897, there are a few indications that Freud still hoped to find evidence to support the seduction theory. Masson even asserted – quite correctly – that the psychoanalytic view was the result of conscious distortion: in the old published versions of the Freud-Fliess correspondence all remarks made by Freud after September 1897 indicating that he might still hope to prove the correctness of his seduction theory have been left out. It is important to stress that Freud gave up the seduction theory not because in September 1897 he had been confronted with contradictory empirical evidence. Rather, he gave up his theory because he could not reach the results that the theory predicted. It is understandable that during the ensuing months, Freud now and then still hoped to find supporting evidence for his theory. Nothing in the evidence so far had forced Freud to give up the theory just at this time: he could just as well have given up earlier, or could have continued his efforts for some more time.¹⁰

Why has no one seen this before?

Our answer to the question why Freud gave up the seduction theory is basically the same as the first motive which Freud himself mentioned in his famous letter to Fliess.¹¹

The literature about why Freud gave up the seduction theory is dominated by two parties. There is a group of authors with feminist sympathies – most notably Jeffrey Masson – and there are psychoanalysts and Freud sympathizers who defend him against the accusations by Masson. According to Masson the Freud of the 1896 seduction theory was a courageous man who made an important

¹⁰ So Freud was correct when explaining in his 1897 letter why he no longer believed in his seduction theory, he mentioned all his *motives* (German: *Motive*) – and not *reasons*, as all the existing English translations have it. The 1954 translation even says: 'reasons for rejecting' instead of 'motives for disbelief'.

¹¹ Using the 1954 English translation of the 1897 letter, E. M. Thornton came to a somewhat similar conclusion:

First, there was 'the continual disappointment of my attempts to bring my analyses to a real conclusion' – this after he had presented his eighteen cases to the Society of Neurologists and Psychiatrists as completed and, by implication, cured cases! (Thornton 1983: 196)

However, Thornton presented this as just one of the many inconsistencies and impossibilities concerning the seduction theory.

discovery; in this view there is no room for a Freud who boasted of success which had not been reached. The psychoanalytic party considers the seduction theory a major mistake, but does not doubt Freud's sincerity.

The correct answer became evident because of the discovery of the correct transcription (not 'meine', but 'eine'), but unfortunately this discovery was made by the wrong people. Masson was the first to publish it, and the psychoanalytic party was the first to publish the correct German transcription. The psychoanalyst-historian Peter Gay in his new Freud biography mentioned the first motive correctly as: 'He [Freud] could not complete any of his analyses' (Gay 1988: 94). However, Gay drew no further attention to this and immediately went on to mention the other motives which Freud listed in his letter. Apparently he did not see the importance of the new transcription; unfortunately neither did the parties who had discovered and published it. These parties have dominated the debate about the seduction theory, and both are handicapped by their feelings of loyalty towards Freud: towards the Freud of 1896, or towards Freud in general.

However, there have been some authors who did stress that the seduction theory was different from the impression given by Freud in his later publications. It is difficult to understand why none of these authors were able to answer the question why Freud gave up his seduction theory.

In 1972 and 1974 Frank Cioffi showed that Freud's later remarks about how he gave up the seduction theory cannot be true. Cioffi's work has not received the attention it deserves. In 1984 he published a long and devastating review of Masson's *Assault on Truth*. He showed that Masson had distorted Freud's seduction theory.

We already mentioned the excellent discussion of the seduction theory Jean Schimek published in 1987. She showed convincingly many contradictions between Freud's later remarks and the actual content of his 1896 theory. She did not pose the question why Freud gave up his seduction theory; she did not use her arguments to cast general doubts on Freud's thinking; she restricted herself to a correction of the dominant view of the seduction theory.

In 1988 Isolde Vetter in a German unpublished MA thesis intelligently surveyed the discussion on the seduction theory. She especially and effectively demolished the remarks made by Masson and by Freud himself. She did use the new transcription ('eine Analyse') but did not draw special attention to its importance.

Why have we never seen this before? For years we had the ingredients necessary for this answer. It was well known that the transcription had been changed: not 'meine Analyse' but 'eine Analyse'. One of us has written about the fraudulent way Freud had always reported Breuer's therapeutic success with Anna O. (Israëls 1985) and about the differences between the old and the new edition of the Freud-Fliess correspondence (Israëls 1989). Maybe our own blindness – just like that of our predecessors – is caused mainly by the fact that it is difficult to detach oneself from a dominant view. With the seduction theory

there were two dominant views – the view dominated by the later Freud, and the view of Masson – and it was difficult to see that neither is correct. Cioffi should especially be credited for seeing this.

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