4

Psychoanalysis and Pseudoscience: Frank J. Sulloway Revisits Freud and His Legacy

Interviewed by Mikkel Borch-Jacobsen

Frank Sulloway received his PhD from Harvard University in 1978 and from 1984–1989 was a MacArthur Foundation fellow. He is currently visiting scholar at the Institute of Personality and Social Research and visiting professor in the Department of Psychology at Berkeley.

Sulloway is widely known among Freud scholars for his comprehensive book of 1979, *Freud, Biologist of the Mind: Beyond the Psychoanalytic Legend.* In this now-classic book, Sulloway provides the first sustained analysis of the role of biology in Freud's work and punctures the widespread claim that Freud was a misunderstood genius working in "splendid isolation." Based on his penetrating reading of Freud's early correspondence with his close friend and ally, Wilhelm Fliess, and on primary research in the history of science, Sulloway cut through decades of mystification about psychoanalysis and forever changed the course of Freud studies. Put simply, Sulloway returned Freud, who was, after all, trained as a neurologist, to his proper intellectual context: nineteenth-century science.

Sulloway has remained interested in psychoanalysis, writing the occasional essay, but has also moved on to his other interests in the history of science. His best-selling book, *Born to Rebel: Birth Order, Family Dynamics, and Creative Lives*, was published in 1996, and he is currently

working on *In Darwin's Footsteps: Discovery and Change in the Galapagos Islands.* He also recently completed a several-year study of the adaptive significance of religion.

The interview with Mikkel Borch-Jacobsen took place in Boston on November 19, 1994, but was edited and updated in 2005. The interview is part of a joint project conceived in partnership with historian Sonu Shamdasani. Borch-Jacobsen himself is interviewed later in this book.

. . .

MB-J: *Freud, Biologist of the Mind: Beyond the Psychoanalytic Legend* marked an important date in Freud studies, not only due to its historical rigor and its impressive erudition, but also because it was in many respects the first overtly "revisionistic" interpretation of Freud and the origins of psychoanalysis. You yourself introduce this term in the first pages of the book, which has come to define the subsequent scholarship that has challenged the Freud legend. It was not that you were the first one to call into question this legend, forged by Freud and his biographer-disciples. Paul Roazen and Henri Ellenberger had already preceded you in taking on aspects of this legend. But you were without a doubt the first to have so clearly and overtly presented your historical work as a critique of psychoanalysis. At bottom, how did you, who are not a psychoanalyst, decide to write an intellectual biography of Freud? At the outset, were you favorable to psychoanalysis? Or did you begin this enormous work already with the idea of putting it in its place?

FS: Let me begin by telling you how I became interested in writing a book on Freud. I had always felt that one ought to know something about psychoanalysis, so when I graduated from Harvard College, in 1969, and was about to begin my graduate school education, I decided to read Ernest Jones's biography of Freud and some of Freud's principal works, such as *The Interpretation of Dreams* [1900]. One aspect that particularly puzzled me about Ernest Jones's biography was that he never really explained the origin of some of the most fundamental concepts of psychoanalysis, at least in the way a historian of science would do. These concepts are mostly taken for granted. The reason why Jones did this is that he assumed these concepts were fundamentally true, just like the laws of gravity: one doesn't really need to explain, in any great detail, how and why Isaac Newton became aware of gravity, for gravity obviously exists.

I wasn't satisfied by this approach. Many psychoanalytic assumptions, such as cathexis, organic repression, and childhood polymorphous perversity, for instance, are not intuitively obvious, and they struck me as having no adequate prehistory to them. So I read Freud's correspondence with his friend and colleague Wilhelm Fliess in the abridged and censored edition that was available at the time. In this correspondence I noticed references to what Freud called abandoned erotogenic zones—the notion that a child would find pleasure in oral and anal sensations, including the smell of feces—together with Freud's remarks that such abandoned erotogenic sensations have phylogenetic implications and links to zoophilia. I recognized these remarks as endorsing the basic assumption behind Haeckel's biogenetic law—namely, that the individual is destined to recapitulate the phylogenetic history of the species—and in this respect Freud's whole discussion made perfect sense to me. What particularly struck me, though, was that these discussions were taking place in December 1896 and January 1897—that is to say, about nine months before the supposed discovery of infantile sexuality during Freud's famous self-analysis. Now, how could Freud "discover" something that he had already been talking about for almost a year? It immediately struck me that these discussions about infantile sexuality were part of a dialogue with Fliess. A Berlin physician who specialized in nose-and-throat disorders, Fliess was also widely read in the field of biology, and he would have understood Freud's implicit endorsement of Haeckelian biogenetic thinking about human development.

Fliess's side of the correspondence is largely lost, but it is quite obvious from Freud's letters that he was not receiving letters back from Fliess saying, "How dare you talk about the child being sexual and having abandoned erotogenic zones!" Ernest Jones presents Freud as a man who made himself very unpopular by talking and writing about infantile sexuality, but this is certainly not the picture one gets from the correspondence with Fliess. Fliess is apparently taking infantile sexuality for granted, and he is a willing partner in this whole discussion.

Given the theoretical assumptions Fliess apparently shared with Freud, it becomes readily understandable why Fliess did not reject such ideas. In Fliess's case, these shared assumptions had also led him to propose that all life is regulated by two sexual rhythms—a 28-day female cycle and a 23-day male cycle [Fliess 1897]. Whether one thinks Fliess's specific ideas are right or wrong—and we now know them to be mostly wrong, especially about

the supposed male cycle—it is clear that he shared with Freud the conviction that all life is regulated by sexuality, and hence by sexual chemistry. In Fliess's 1897 book, which Freud read in manuscript in 1896, before his self-analysis, Fliess had argued that the child's date of birth is determined by the fluctuations of these two sexual cycles. Fliess also claimed that major stages in childhood development were influenced by these cycles, so it was perfectly natural for him to believe in infantile sexuality.

Now here I was, face-to-face with an interesting problem as a young historian of science. I had come across a famous intellectual "discovery" occurring nine months before it is supposed to have taken place in Freud's life. I had also come to understand that this discovery had emerged in the context of a collaborative relationship between two people, one of whom—Wilhelm Fliess—had been consistently maligned by Freud's biographers for his pseudoscientific views about periodicity and bisexuality, and whose deductions about infantile sexuality had never been mentioned in any previous biographical writings about Freud.

When I came to this realization in the early 1970s, I thought this to be a very strange state of affairs, so I did something that few people had probably done since the Freud-Fliess letters came out in the German-language edition of 1950. I went back and read Wilhelm Fliess's original works. Lo and behold, there I found references to the child having erections at 23- and 28-day intervals, to thumb-sucking being a substitute form of sexuality, and so forth. My God, I thought, this material suggests a completely different view of the origins of one of Freud's most fundamental insights—the existence of infantile sexuality! So I started to write a short paper on this topic. It grew into a long paper, then into a short book, then into a medium-sized book, and eventually into the fairly substantial book I finally published in 1979.

The reason why my manuscript kept growing is because this biogenetic way of thinking about sexuality, far from being a single, accidental episode in Freud's intellectual development, turned out to be a pervasive and unifying theme in the development of his overall psychoanalytic theorizing. The more I followed the thread of these particular conceptual threads throughout Freud's life and thought, the more I realized that the psychobiological paradigm that Freud and Fliess had shared in the 1890s was a mode of thinking that had subsequently sent its tentacles through the entire creation of psychoanalysis as we know it. I found myself in an

odd situation, for I had no intention initially of writing a book on Freud. Once I got started, however, the book essentially wrote itself.

As soon as I realized there was a fundamental discrepancy in the traditional historical accounts of how Freud made his most important discoveries, this conclusion opened up Pandora's box, as it were. You asked before about my original attitude toward Freud, and I would have to say that the writing of my book radically transformed my thinking about him. When I began the book, I approached Freud, as most people did at the time, as one of the great minds of the twentieth century, somebody on a par with Copernicus and Darwin, as he himself once claimed. But the more I looked into the development of psychoanalysis, the more I discovered that it was based on outmoded nineteenth-century assumptions that were clearly refuted by the rediscovery of Mendel's laws in genetics, by the overthrow of Lamarckian theory in evolutionary biology, and by the rejection of the various Helmholtzian physiological assumptions that were crucial to Freud's thinking about hysteria, and neurotic symptom formation more generally.

So when I finally finished this book, I found myself somewhat reluctantly having to admit that Freud was not the great discoverer I—and many others—had thought. I became, in spite of myself, a critic not only of psychoanalytic theory but also of what I increasingly saw as the politically motivated act of construction, by Freud and his followers, of a historical legend to prevent this view of Freud from being widely understood. In this general critique of the Freud legend, I was, of course, following a path on which others had gone before me, in particular Henri Ellenberger, on whom I drew considerably in my book.

MB-J: How was the book received?

FS: On publication, my book was widely perceived as a far-reaching critique of the validity of psychoanalysis, although I still had to admire Freud for the sweep of his overall intellectual creation. Had the outmoded biological assumptions on which he built his psychoanalytic edifice been correct, then psychoanalytic theory might well have been mostly correct. I tried to treat Freud, in my book, the way a historian of science would treat Aristotle and any other great thinkers of the past who were now known to be mostly in error in their scientific theories. This particular historical approach, incidentally, seeks to avoid the fallacy of Whiggish history, or the tendency to write history from the perspective of how it all finally turned

out. It was a methodological perspective I had been taught in graduate school. For example, historians of science were then trying to write the history of pseudosciences, such as phrenology, without being overly dismissive of these intellectual pursuits, given the obvious evidence of their ultimate scientific failure. In hindsight, I've come to realize that such an anti-Whiggish historical approach has its own limitations, because I was in many ways too kind to Freud and sometimes wrote about his "insights" and "discoveries" without specifically stating that I did not personally believe these specific insights and discoveries to be valid. Frederick Crews [1986, 88–111] later took me to task for using such language, and he was right to do so, although he himself, I think, sometimes went too far and occasionally committed the fallacy of Whiggish history in his own criticisms of Freud.

Since I wrote my book on Freud, I have become, as you know, even more critical of Freud's theories and legacy. Much of this subsequent critique is already implicit in my book, but it was not developed as clearly as it should have been, in part because of my attempt to avoid the fallacy of Whiggish history. In any event, I have come to see psychoanalysis much more clearly as something of a tragedy, as a discipline that evolved from a very promising science into a very disappointing pseudoscience. Science is a two-step process. The first step is the development of hypotheses, and at this stage it doesn't really matter whether one's hypotheses are right or wrong. In other words, Freud could afford to have a lot of erroneous hypotheses based on ideas and assumptions that were current in his day but that later turned out not to be true. This is not the failing point of good science. Where science more often goes wrong is during the second step, which consists of testing one's hypotheses and giving them up if they turn out to be wrong. This second step is much more crucial, in fact, than the first one, for one can afford to be mistaken during the first step only so long as one is extremely rigorous during the second step.

The more I studied psychoanalysis, especially in its clinical practice as described by Freud in his famous case histories, the more I came to the conclusion that Freud had developed a set of extremely compelling, extremely plausible hypotheses for his day, but that he never took that key, second procedural step in the rigorous manner that is required for true science. Science is not just a set of facts and theories but also a method, a way of questioning what one thinks is true. And it's in its faulty methodology that psychoanalysis has met its ultimate downfall.

MB-J: You claim in your book that Fliess's theories on bisexuality and infantile sexuality had a completely determining influence on Freud. You also argue that it was with these ideas from Fliess that Freud could fill the void left by the collapse of his seduction theory and substitute it with a theory of sexuality of biological inspiration. Would you go so far as saying that it was Fliess who was the veritable instigator of that which we call Freudian psychoanalysis?

FS: No, I wouldn't—and I didn't say that in my book. In my book, I described the relationship between Fliess's ideas on infantile sexuality and what became of them in psychoanalytic theory in terms of a transformation. The title of one of my chapters is "Freud's Psychoanalytic Transformation of the Fliessian Id." Freud clearly saw implications in this psychobiological view of infancy that Fliess had not, and this was a very creative transformation indeed—which doesn't say anything, by the way, about whether it was right or wrong. However, the core assumptions of Freud's theory of sexuality, and psychosexual development more generally, clearly drew on ideas that he shared with Fliess and in some cases derived directly from him.

Indeed, these Fliessian assumptions were crucial to salvaging Freud's theory of human sexual development and of psychopathology, once the seduction theory fell apart. If neuroses are not due to childhood sexual traumas—to "seductions," as Freud called them—but rather to endogenous, internal impulses that either do or do not undergo repression, this new way of thinking obviously throws the emphasis back onto the spontaneous nature of sexuality in the child. This is clearly a biological view about human sexual development; and indeed, in his later works, Freud more than once pointed out the parallels between the psychoanalytic view of the endogenous nature of infantile sexuality and Fliess's own biological theories. Like Fliess, Freud explicitly wrote about infantile psychosexual development in terms of the periodic ebb and flow of sexuality in a child, and he also proposed that night terrors in children, which he believed to be caused by improperly channeled libido, occurred at regular, 28-day intervals. Whether or not Freud believed in the correctness of Fliess's theories in later years is unimportant, although there is no evidence that Freud ever abandoned his belief that Fliess was right about the fundamental role of periodic developmental "thrusts" in infancy and life more generally.

What ultimately mattered for Freud and psychoanalysis is that Fliess's way of thinking about human development as being biologically driven was crucial to the new view of human development that Freud adopted once the seduction theory and his predominantly environmentalist interpretation of neurosis fell through in 1897. In fact, Fliess's views must have helped to undermine Freud's confidence in that erroneous theory. The resulting change in Freud's thinking involves extensive intellectual borrowing, and the fact that Freud developed these ideas into a vast intellectual system doesn't minimize his considerable debt to this Fliessian way of thinking.

Could Freud have done it without Fliess? It is difficult to second-guess history, and it is also true that Freud was himself sufficiently versed in evolutionary and biogenetic views of life to have reached the same series of insights by himself. But it certainly didn't hurt for him to have someone actively pushing these key biological perspectives at a time when he desperately needed an alternative to his failed explanations for neuroses. In the history of science, there is clearly an important distinction between ideas just being out there and one's best friend touting these very same ideas that later became central to Freud's own theories of human development and the origins of the psychoneuroses.

MB-J: Psychoanalysts claim that Freud drew his ideas from two sources: from clinical observation of his patients and from his famous self-analysis. By contrast, you show the decisive role of Freud's reading, which, at the very least, greatly qualifies the decisive role that has been attributed to the former sources and further raises the suspicion whether the latter, rather than being a world-historical act of introspection, largely consisted of a prolonged sojourn in the library. How do you view the significance attributed to Freud's self-analysis in the genesis of psychoanalysis?

FS: I have always felt that self-analysis was not the fundamental cause of Freud's abandonment of the seduction theory or of his subsequent theoretical developments. The handwriting was on the wall. Fliess's biological viewpoint, the evidence from sexology, the disappointing clinical evidence from his own quasi disciple, Felix Gattel, who had been working up case histories from a Freudian perspective in Richard von Krafft-Ebing's clinic, and so forth—all these different sorts of negative evidence indicated that the seduction theory just wasn't true. Rather than discovering that

unwelcome truth in his self-analysis, Freud essentially read into his self-analysis what he had already begun to realize from these other sources of evidence.

MB-J: Fliess would later accuse Freud of reading his own mind into his patients'.[1] Are you saying that, in this instance, Freud read Wilhelm Fliess's, Richard von Krafft-Ebing's, Albert Moll's, and other sexologists' theories into his own mind?

FS: Yes, I would say he did just that. How is it possible, in a self-analysis, not to be conditioned by all the scientific knowledge, reading, and diverse evidence that a person like Freud has gathered from numerous other researches and disciplines? How could one possibly prevent those relevant sources of information from steering one's self-analysis in a certain direction? If one begins to read in the scientific literature that the infant is much more sexually spontaneous than one had ever thought, how could one not decide to probe that issue in one's own self-analysis? So it shouldn't come as a big surprise if Freud, during his self-analysis, supposedly uncovered a memory of having seen his mother naked at the age of two. If some of the books Freud was reading were telling him similar things, and if he then discovered such experiences in his own childhood, well, big news! It's obvious, and hardly profound.

In traditional Freud scholarship, the self-analysis has been made into a causal agent of Freud's originality, but that historical scenario is simply not true. Ideas that were supposedly derived from the self-analysis are credited for many of Freud's most important intellectual discoveries, but we now know that those ideas were generally derived from somewhere else and were definitely not the product of the self-analysis per se. The self-analysis is one of the great legendary stories in the history of science. Although Freud himself really didn't spawn this aspect of the Freud legend, it is interesting to note that he did nothing to prevent it from spreading.

It was Fritz Wittels who first claimed, in his 1924 biography of Freud, that Freud must have discovered infantile sexuality in the course of his self-analysis. Freud read that biography very carefully and corrected various errors in it, but he did not correct this one. The reason he didn't, I think, is that he rather liked the story. It clearly was not true, but it was the kind of biographical story that ought to have been true according to psychoanalytic theory and the legend its theory entails.

1. "The reader of thoughts merely reads his own thoughts into the other people" (in Masson 1985, 446).

MB-J: You are a historian of ideas, and at bottom you seem to hold that ideas engender one another in a continuous sequence. Some people believe in radical scientific "revolutions" or "breaks." Hence, in the case of psychoanalysis, simply because Freud took a concept from Fliess, Krafft-Ebing, or Albert Moll doesn't mean that he is speaking of the same thing these other people were. This line of thinking is very fashionable amongst French psychoanalysts of Lacanian inspiration, and Elisabeth Roudinesco [1986] explicitly poses it against you as follows: "Sulloway forgets a fundamental epistemological fact: Freud is not content with establishing what everyone else knows in 'stealing' ideas from his contemporaries, he translated facts through new concepts . . . In accusing him of falsifying history, Sulloway reasons, despite his great erudition, as if theory was of the same nature as concrete facts, as if the concept of a dog was produced by barking. This type of argumentation is frequent, and has to be put back in history, as one of the forms of the resistance to the Freudian discovery. This consists in demonstrating that Freud invented nothing, and that of which he speaks existed before him, like the most shared thing of the world" [32–33]. How do you reply to this?

FS: This is an interesting historiographical issue, and a delicate one as well. At the simplest level, I would answer your question this way: if one totally believes in psychoanalysis and one thinks Freud is a genius, then everything he did is seen to be revolutionary, and any parallels with his predecessors have to be the product of an amazing transformation of ideas, of a radical rupture from the past. The more one reveres Freud, the more one is bound to endorse a revolutionary model of history. By contrast, if one is critical of Freud and psychoanalysis, it is natural to see the history of psychoanalysis as an evolutionary process, with substantial intellectual debts to previous thinkers.

Now, in the quotation from Roudinesco that you just cited, I am portrayed in a rather extreme historiographical manner as claiming that Freud lifted hook, line, and sinker most of his ideas from other people. I certainly didn't say this in my book, and I went to great lengths in my book to show the many ways in which psychoanalysis synthesized existing ideas and then transformed them in interesting ways. Roudinesco is attacking a straw man—a position I have never held. Again, one of the fundamental chapters of my book deals with "Freud's Psychoanalytic Transformation of the Fliessian Id." There is no revolution in the history of science where

there is complete conceptual discontinuity; there is always some degree of transformation, as I. Bernard Cohen [1985] has persuasively argued. As historians of science have generally come to appreciate in their study of other great scientific innovators, such as Galileo, Newton, and Darwin, an evolutionary model of history is almost always closer to the truth than a revolutionary one—even for revolutionary thinkers. So it's really a question of exactly where we put Freud on a scale extending from total borrowing of preexisting ideas to radical innovation.

Personally, I would put Freud somewhere in the middle on such a scale of originality, but rather closer to the original end than the unoriginal end of the scale. I give him tremendous credit for having transformed old ideas in new ways, even though most of these new ways of thinking were not scientifically correct. Psychoanalysis is an impressive synthesis of ideas from the late nineteenth century, and nobody else had put all these ideas together in quite the same powerful way that Freud did. If we lived in a Lamarckian world rather than in a Darwinian world, if the inheritance of acquired characteristics was really possible, and if energy really did circulate in the body according to a Helmholtzian, hydraulic model so that unused libidinal energy could become diverted and cause neurophysiological disturbances—in a word, if all of these mistaken biological notions were true, it is quite possible that psychoanalytic theory would, in large part, be true. The problem is that these assumptions are not true. The discipline of psychoanalysis was built on intellectual quicksand, and it has been sinking ever since.

In any event, if people write on the subject of Freud's originality from a loyalist psychoanalytic perspective, they will almost always defend the "rupture" view of history, and they will tend to portray anyone else who takes the evolutionary view as trying to wheedle Freud's originality down in an unflattering manner. That kind of disagreement is part of the politics of the historiographical discipline. Incidentally, I tried to analyze this politics at length in my book, devoting a chapter to "The Myth of the Hero" and attempting to show in this chapter how Freud and his followers touted a revolutionary model of history in an effort to enhance his originality and to insulate psychoanalysis from its origins in outmoded nineteenth-century psychobiology.

MB-J: Following up on this last point, you affirm in your book that psychoanalysis is not the purely psychological theory that Freud and his

successors wished to depict it as. On the contrary, you state that there is a profound continuity between the initially neurophysiological and biological preoccupations of Freud and the complex psychobiology that he elaborated under the name of psychoanalysis. If what you say in your book is true, how, then, could everyone be persuaded that Freud was a pure psychologist who had broken with the biologism of his contemporaries? How did this legend establish itself? And why?

FS: This is a very interesting question because it goes to the heart of the politics of the psychoanalytic movement and the way this movement has sought to portray itself and its history. It is fair to say that if one goes through the *Standard Edition* of Freud's works, one can find him making every possible statement about the conceptual relationship between psychoanalysis and biology. So depending upon what position one wants to take, as a psychoanalyst or as a critic of psychoanalysis, one will find at one's disposal all of the seemingly compelling quotations from Freud that one needs to support one's position. The real question is, why is Freud saying all these obviously contradictory things? When somebody says things that are so contradictory, one has to wonder what is really going on.

The most straightforward answer to this question is that Freud was incredibly ambivalent about the whole issue, and this ambivalence clearly had to do with his troubled relationship with Fliess, with whom he shared a profound desire to unite biology with psychology in an effort to understand human development. As Robert Merton [1976] has shown, ambivalence is a hallmark of scientists' attitudes toward priority. Scientists try to be modest and humble, but they also seek to gain maximum credit for their ideas. Freud's relationship to Fliess was filled with such intense ambivalence over matters relating to priority. The relationship with Fliess is not the only source of Freud's ambivalence about his extensive intellectual debts to biology, but it certainly made a major contribution to it. Freud was also ambivalent about biology for another good reason. His often speculative biological assumptions risked refutation by the progress of science.

Just think of it: Freud had gotten himself into a richly collaborative relationship with this really smart guy who had a lot of provocative and novel ideas about human sexual chemistry, sexual periodicity, bisexuality, and even infantile sexuality that were all helping Freud to solve some of the most significant theoretical problems he was facing at the time. All of

a sudden the issue comes down to, hey, who deserves the most credit? By 1901, we know that matters had gotten to the point where Freud was willing to offer coauthorship to Fliess of what became the *Three Essays on the Theory of Sexuality* if that enticement would ensure continuation of the collaboration [see Masson 1985, 448]. In his letter offering Fliess coauthorship, Freud states that he is planning to entitle this book *Bisexuality in Man.* Clearly, one does not offer coauthorship of one of one's most famous books to somebody else unless one has really merged minds with that person, owes this other person a substantial intellectual debt, and, as was true of Freud, needs this person's continuing assistance with as-yet unsolved problems. The subsequent estrangement between Freud and Fliess prevented this collaborative publication. But the evidence of its far-reaching impact on psychoanalysis is documented in my book, and is based on Freud's correspondence with Fliess, Freud's various published references to Fliess's theories, and Fliess's own published references to Freud's work.

After the break with Fliess occurred, Freud discovered that many of his early followers, such as Stekel and Adler, were also reading Fliess's works—and similar works by other psychobiologically oriented authors. One has to understand that Freud and Fliess were not the only guys in town playing the game of trying to reduce psychology to biology and trying to show how one can fruitfully cross from one field to the other. This was the intellectual game of the late nineteenth century—the game of the Helmholtz school of medicine and later an increasingly Darwinian game, including a game played by many of the people in the emerging field of sexual psychopathology. Trying to unite psychology with biology was an exciting intellectual strategy to pursue, and people are still pursuing it today; for one cannot base a theory of the mind on pure psychology. Such a theory of mind has to have some roots in developmental biology, genetics, neurophysiology, and evolutionary biology.

Freud, of course, knew this because he was trained as a biologist. But with some of his early disciples trying to create alternative schools of psychoanalysis based on their own use of Fliess's and other contemporary researchers' psychobiological works, Freud soon realized that his own previous mining of this intellectual treasure trove was a double-edged sword. If everybody continued to do what he had done by building on biological bedrock, one might soon have a myriad of different forms of psychoanalysis, and then what would become of Freud's own originality and, especially,

his intellectual hegemony in psychoanalysis? Freud therefore decided that the safest thing to do was to tell his disciples that psychoanalysis needed to become a pure psychology. One of the most amazing things is that most of these disciples bought into this requirement; or at least this perspective was generally accepted by those disciples who remained within Freud's circle. These followers really did believe that psychoanalysis was a purely psychological discipline in which the principal sources of evidence are patients on the couch and whatever subjective, self-analytic material one can cull from oneself or from the personal experiences of one's colleagues. This new emphasis on psychoanalysis as a pure psychology, and hence on the discipline being independent of other fields of inquiry, really helped to prevent dissension. I was just rereading the other day Freud's 1926 essay, "The Question of Lay Analysis," in which he proposes to divorce psychoanalytic education from training within medical schools. Now why would he want to do that? Medical schools are the place where medicine has always been taught. But such schools are also the same places where one learns biology and other relevant fields of natural science, which are the cornerstone of modern medicine. Because biology was a constant threat to Freud, the easiest way for him to minimize that threat was for him to privatize psychoanalytic training, to take it out of the medical schools, and to induce his disciples to believe that psychoanalysis is a pure psychology that stands apart from other disciplines and so does not have to answer to them. This strategy did not prevent Freud from going back to biology whenever he felt like it, such as when he developed his ideas about the death instinct in 1920. Like many creative people, Freud told his disciples one thing and then did another himself. But Freud's strategy did immunize his theories from subsequent and much-needed revision when the assumptions on which he had built his psychoanalytic edifice underwent significant revision in the fields from which he had drawn.

Freud's ambivalence toward biology explains why every possible statement about the relationship between biology and psychoanalysis can be found in Freud's collected works. To make sense out of such conflicting statements, one needs to know who the audience is for this or that particular statement.

MB-J: In your view, were all these legends we have been talking about deliberately fabricated by Freud and his successors? Could one go further and speak of dishonesty as regards the manner in which Freud rewrote his own history?

FS: As a historian of science who has studied the lives of eminent scientists such as Copernicus, Galileo, Newton, and Darwin, I am familiar with numerous legends by which to compare the legend about Freud. From this perspective, I will unhesitatingly say that no legend in the history of science has ever been developed in such elaborate ways as the legend fostered by psychoanalysts about its own origins. Psychoanalysis is the only theory in the history of science that demands that its own history be absolutely consistent with the theory developed by its originator. Darwin did not claim, for example, that the discovery of natural selection was the result of a "natural selection" of ideas going on in his head. Newton never claimed that his thoughts "gravitated" toward the theory of universal gravitation. But psychoanalysis demands that the life of its founder—especially his childhood and the heroic, self-analytic path he supposedly took to his discoveries—agree with major tenets of the theory. From a historiographical perspective, this kind of circular logic can get one into a lot of trouble. If Freud's theories were 100 percent true, one might perhaps be able to produce a reasonably good history using this conceptual approach. But, to the extent that the theory is problematic, one is bound to end up with a problematic history—and, most likely, a badly flawed, self-serving history.

This extraordinary requirement—that the history of the theory's origins be explained by the actual theory—created a problem for the history of psychoanalysis that has never been faced by any other discipline in the entire history of science. In this connection, let me now address your related question: to what extent is the psychoanalytic legend tendentious? As I have said, the Freud legend certainly is more fully developed and more politically motivated than any other legend in the history of science, so we can definitely see the hand of motivated distortion in its history. Keep in mind that psychoanalysis was under heavy scientific attack as this legend was taking form. The legend was part of the movement's defense mechanisms. Of course, one might argue that this circumstance was also true of other controversial theories, such as Darwinism. But Darwinism triumphed: people soon realized Darwin was right, and no serious scientist nowadays doubts the fundamental truth of evolutionary theory. Although legends did arise about Darwin, they were never essential to protecting his theories, to immunizing them from criticism. As a discipline, psychoanalysis never succeeded in the way Darwin's theories have done, and the role of the Freud legend has therefore remained politically expedient in the disci-

pline, which also means that there is more motivation, even today, for partisans of this theory to cross the line into tendentious uses of history.

I am not saying, though, that Freud and his disciples sat around a table and deliberately decided to lie about their history. The process was much more subtle. In some cases, these various component myths that make up the Freud legend—of which I have identified more than twenty in my book—were almost innocent because, in the context of psychoanalytic theory, they seemed so plausible. At any rate, such myths were generally not explicitly dishonest. But such legendary forms of history did involve pervasive self-deception. Whenever self-deception is involved, it is always hard to know how much it includes outright dishonesty, as Allen Esterson [1993] has noted about Freud's often blatantly false clinical assertions. This issue is like asking whether there was dishonesty in the bitter political fights that went on in the French National Convention, during the French Revolution, when these deputies portrayed each other in highly distorted ways and often sent each other to the guillotine. The point is that each side believed its own distortions. A. A. Brill has described the ways in which the early disciples at Bleuler's Burghölzli Mental Clinic would analyze each other every time one of them did anything out of the ordinary, such as dropping a spoon or forgetting a name. Well, when one is writing one's own history in this same fashion, living and breathing the theory that informs one's entire sense of history, one is bound to come up with dubious and self-serving conclusions.

MB-J: Is psychoanalysis allergic to history?

FS: Yes, well put. Psychoanalysts do seem to have antibodies toward history, and one important reason is that, in psychoanalysis, nothing is supposed to be the way it seems. The manifest content of thoughts and dreams, for example, is always just a superficial, distorted layer of the latent, or hidden, content. So the job of a psychoanalyst who approaches history is often to show how most of what a nonpsychoanalytic historian has ever written about the subject—whether it is the history of the psychoanalytic movement or some other aspect of psychohistory—has missed the point and is therefore wrong. So far, the rather dismal record of psychohistory, as David Stannard [1980] has shown, is largely a record of gross distortions and embarrassing historiographical failures.

If a fundamental tenet of one's "scientific" way of thinking is that nothing is the way it seems, one soon gets to a point where nothing can be proved, for no evidence can ever be trusted—except the evidence that

confirms what one already believes. If I produced compelling historical evidence about, say, some idea that Freud derived from Richard von Krafft-Ebing, the average psychoanalyst who wanted to defend Freud's originality might think: "Ah, but that's only superficial evidence—manifest-content evidence! Since Freud's use of that idea he supposedly derived from Krafft-Ebing was substantially different once Freud developed his revolutionary psychoanalytic vision, Freud is really an original genius, not an intellectual pilferer. So, you see, it's not at all what it seems." Unfortunately, psychoanalytic reasoning is too circular for its practitioners to correct such self-serving accounts of history, or go beyond them.

MB-J: Tell me a bit more about how your views regarding Freud and psychoanalysis have changed over the years since you wrote your 1979 book.

FS: When I undertook my book on Freud in the mid-1970s, I researched and wrote it as a historian of ideas. I approached psychoanalytic theory as an intellectual system, tried to show where these ideas came from, sought to trace and dissect the various conceptual components that Freud adapted from other people's work, and attempted to support my historical assertions by a detailed study of Freud's marginalia in his personal library, and so forth. But I didn't tackle psychoanalysis as a system of clinical treatment or as a form of scientific training. A decade later, it had become much clearer in my mind that my failure to include a chapter on psychoanalysis as a clinical method, and also as a form of medical education and practice, represented a significant omission from my book, which is something that I acknowledged in a 1991 article on Freud's case histories. Indeed, when one looks closely at psychoanalysis as a form of clinical practice, one is bound—in my view at least—to become much more critical of Freud's achievements and legacy.

As I stated earlier, science is a two-step process. The first step is to formulate reasonably plausible hypotheses—the best one can propose under the circumstances. The second step, which is the really crucial one, is to test these hypotheses and to accept defeat when they are shown to be incorrect. This is an extraordinarily difficult thing for human beings to do, and it took a revolution in science in the seventeenth century—the so-called scientific revolution—to develop an intellectual technology that was ultimately accepted by the entire scientific community in an effort to make its practitioners more self-critical about the foundations of scientific knowledge. This is an intellectual technology that consists of constantly throw-

ing all one's pet theories to the wall, so to speak, and it is something that people do not learn without extensive professional guidance and training. Even with such training, the scientific method is difficult to implement, because we always tend to favor our own hypotheses, as Darwin once pointed out. In his *Autobiography*, Darwin noted that he had tried to follow "a golden rule, namely, that whenever a published fact, a new observation or thought came across me which was opposed to my general results, to make a memorandum of it without fail and at once; for I had found by experience that such facts and thoughts were far more apt to escape from the memory than favourable ones" [Darwin 1958]. Unlike Darwin, Freud was less scrupulous about following this "golden rule," and his faulty clinical methods also continuously undermined his ability to do so.

Controversy over Freud's theories only made matters worse. What did the field of psychoanalysis do, during its early years, when it ran into trouble—that is, when it was confronted by ever-mounting criticisms from psychiatrists, psychologists, and people in the biological fields from which Freud had borrowed so extensively? The field reacted regressively by privatizing its training mechanisms, which means that it took itself out of that enormously successful tradition, which first emerged during the scientific revolution, of testing theories using formal methods of self-criticism. Instead, the discipline of psychoanalysis took a step back toward scholasticism and the medieval tradition that preceded the scientific revolution by founding small private institutes in which knowledge could be transmitted dogmatically and where students were taught how to overcome their "resistances" to the theory. Edward Glover [1952], who directed research at the London Institute of Psychoanalysis for sixteen years, has highlighted the enormous pitfalls of the training analysis:

> It is scarcely to be expected that a student who has spent some years under the artificial and sometimes hothouse conditions of a training analysis and whose professional career depends on overcoming "resistance" to the satisfaction of his training analyst, can be in a favorable position to defend his scientific integrity against his analyst's theory and practice. And the longer he remains in training analysis, the less likely he is to do so. For according to his analyst the candidate's objections to interpretations rate as "resistances." In short, there is a tendency inherent in the training situation to perpetuate error.

If you think about it even for a moment, this form of education is the most astonishing reversal of everything that Copernicus, Kepler, Galileo,

Newton, and the entire scientific revolution fought for! Once I fully appreciated how regressive a step this was, seen in historical context, I decided to look at Freud's own case histories to see to what extent he ever really tested his hypotheses. You see, as long as one concentrates on the first, or hypothesis formulation, stage of science, which is basically what I had done in my book, Freud looks pretty good. He was a genius at formulating plausible hypotheses, and he deserves an A for that achievement. But when it comes to the second, or testing, stage of science, he gets no more than a D– or even an E. He was personally responsible for the privatization of training mechanisms, and this privatization is equivalent to ceasing to test—in other words, for rejecting the hard-won scientific strictures of the last four centuries, and hence the most important achievement of the scientific revolution.

Psychoanalysis may have been a science in 1895 or perhaps even as late as in 1900, but by 1915 or 1920—that is, by the time it had developed the training analysis as a routine form of psychoanalytic education—the discipline could no longer claim to possess any real scientific pretensions. Through its rigid forms of training, psychoanalysis ceased to be a science, and when a discipline ceases to be a science, it becomes a pseudoscience. I have not the slightest doubt that psychoanalysis is a pseudoscience today. It's not that psychoanalytic theory is untestable, by the way. Many components of the overall theory are definitely testable, as Adolf Grünbaum [1984] has argued. The problem lies with the field's practitioners, who were not taught to test their theories in a scientific manner. So psychoanalysis per se is not technically a pseudoscience. Rather, its practitioners are mostly pseudoscientists—an important distinction, although the net result is that the field generally espouses pseudoscientific ideas and is unable to correct them.

MB-J: In the article you mention, you don't hesitate to put in doubt the veracity of all the great case histories of Freud, drawing on the damaging discrepancies brought to life by Morton Schatzman, Han Israëls, Zvi Lothane, Patrick Mahony, and Karin Obholzer, not to mention Paul Roazen, Ola Andersson, Henri Ellenberger, and Peter Swales. Was the great Freud a charlatan? To what extent can one still trust the veracity of the factual evidence that Freud marshals in support of psychoanalysis?

FS: I wrote my 1991 article on Freud's case histories partly as a missing chapter of my book, and partly because of a series of valuable studies that had appeared in the meantime on various specific case histories. In the light of this new and intriguing literature, I felt it was time for someone to

try to synthesize what we had learned from it. So I am grateful to all those scholars you just mentioned for having done so much of the spade work, and to people like Frank Cioffi, Adolf Grünbaum, Frederick Crews, and Malcolm Macmillan, who have also made important contributions to our understanding of Freud's methodological shortcomings. What I tried to do in my article, based in large part on the research by these scholars, was to see whether there was some kind of larger pattern in these case histories, and indeed there was.

Previously, looking at Freud as a theoretician, I had not fully appreciated the extent of his arbitrariness as a practitioner and how wide a berth there was for him to interpret, in arbitrary ways, the reasons for anything said to him. Yes, I certainly did understand this point in a general way, as when I wrote in the concluding chapter of my Freud book: "Time and time again, Freud saw in his patients what psychoanalytic theory led him to look for and then to interpret the way he did; and when the theory changed, so did the clinical findings" [1979, 498]. But I simply did not do full justice, in my treatment of Freud, to detailing the faulty clinical and intellectual methods that underlay these flawed interpretations.

Here is just one clinical example, to give a sense as to what psychoanalytic interpretations by Freud were often like. Clarence Oberndorf went to see Freud around 1923 or 1924. Like all candidates for training under Freud, Oberndorf came in prepared with a dream. The dream involved driving in a carriage with a black horse and a white horse. Because Oberndorf came from the southern part of the United States, Freud interpreted the dream to mean that Oberndorf had an inhibition about whether he should marry a white woman or a black woman. Oberndorf and Freud haggled over the meaning of this dream for a couple of months until Freud just got fed up with Oberndorf's "resistances" and brought the analysis to an end. If one goes back through all of Freud's case histories, one finds a similar pattern of patients reporting how astonished they were at the seemingly arbitrary conclusions Freud reached, and how Freud stubbornly resisted objections to his own formulaic psychoanalytic interpretations. Of course, this was nothing new. The fiasco of Freud's seduction theory arose in the same manner [see Esterson 1993].

To provide another salient instance, the Wolf Man has described Freud's interpretation of his famous dream about wolves as "terribly far fetched" [in Obholzer 1982, 35]. He also complained that Freud insisted

that he would one day remember the events that had made him ill, but he never did. Even more telling, the Wolf Man told Karin Obholzer that his dream was not about wolves at all, but about white dogs. It would not have sounded nearly so good for Freud to have called his famous patient the Dog Man, so he became the Wolf Man! This convenient transposition allowed Freud to bolster his dubious clinical analysis with arguments about wolves drawn from folklore.

Freud's case histories are littered with these kinds of discrepancies, which generally arise from an overzealous effort to make the facts fit the theory. Once one has collected and examined them all, and has fully realized how loose the inference generally is by which Freud goes from such clinical facts to speculative inferences, then to theory, one begins not only to question the whole undertaking but to repudiate it entirely. More often than not, Freud came up with interpretations that were astonishingly implausible, which his patients understandably did not accept and which sometimes involved blatant distortions of the facts.

All of this explains why Freud had so much trouble establishing a discipline in which disciples could ever come up with the same theoretically "correct" interpretations he did. His interpretations were sometimes so arbitrary that there was rarely any real consensus. As I have argued in my article about Freud's case histories, this is a significant part of the reason why Freud felt the need for privatizing psychoanalytic training. Given that his interpretations could neither be replicated nor proven, there was no other effective way of getting agreement with other psychoanalysts other than by institutionalizing a formal process by which "resistances" could be completely eliminated from the minds of psychoanalytic candidates. That process was the training analysis.

MB-J: If what you're saying is true, it would mean that Freud substituted an initiatory, cultlike process for the scientific mode of reproduction of knowledge. Would you go so far as to say that Freud, under the cover of elaborating a science of the psyche, in reality founded a new religion founded on the blind adhesion to several founding myths?

FS: Freud would have been shocked to hear this said about him, but I do think that psychoanalysis fulfills all the needs that religions used to fulfill and that it has also adopted some of religion's institutional features. Much of psychoanalysis's appeal is that it provides an answer to almost everything, and, in this respect, there is no modern scientific theory quite

like it. It makes Darwinism, which certainly explains a lot about the world, look like a paltry and rather specialized philosophy. What can one really achieve by applying natural selection to one's own life and problems? Not much, at least on an individual psychological level, or at the level of incredible detail that Freud aspired to explain. Although much progress has been made in the field of Darwinian psychology, this approach to human behavior still cannot hold a candle to psychoanalysis in terms of its explanatory scope for individual behavior.

When one is in possession of a theory that explains just about everything, almost nothing is refutable anymore, and what one ends up with is more like a religion or a pseudoscience than a science. As I view it, psychoanalysis is just such a pseudoscience, and it cannot be denied that it also has all the properties of a religion. There is a wonderful article written in the 1970s by George Weisz [1975], dealing with the sectarian properties of psychoanalysis, and I don't think anybody has improved on that insightful analysis. Even Freud's own disciples, such as Hanns Sachs or Max Schur, talked openly about the sectlike qualities of the psychoanalytic community. By the way, it is not uncommon in the history of science to see people banding together in alliances of power, coordinating their responses to critics, founding new journals, and so on—especially in the incipient stages of a new theoretical discipline. But this kind of behavior, which one can clearly associate with sectarianism, rarely becomes the be-all and end-all of each new field's way of constructing knowledge. Psychoanalysis, on the contrary, never outgrew these sectarian tactics. The principal reason, as I have said, is because this churchlike construction of knowledge is the only real way to obtain clinical agreement among psychoanalysts. If the psychoanalytic community was not socially constructed, through the training analysis and the inherently unscientific frame of mind it inculcates, there would never be any real consensus at all.

In short, what is wrong with the discipline of psychoanalysis is not just the theories. False theories can always be discarded if the underlying methods are sound. The greatest failing of psychoanalysis is its blatant rejection of the scientific method. Without such methods for critical thinking, a discipline inevitably drifts from one pseudoscientific system of belief to another. That, in my opinion, is Freud's most tragic legacy.