

Was Freud's core identity that of a natural scientist? Freud's drawing of the spinal ganglia of *Ammocoetes* (*Petromyzon planeri*), a primitive form of fish, for his 1878 monograph on the subject is adduced in support of this thesis by Frank J. Sulloway during his conversation with Peter L. Rudnytsky.

PSYCHOANALYTIC CONVERSATIONS

*Interviews with Clinicians,
Commentators, and Critics*

Peter L. Rudnytsky

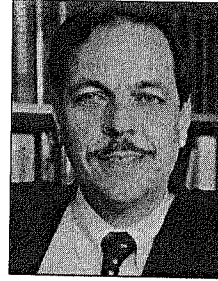


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Frank J. Sulloway

On Darwin and Freud

Peter L. Rudnytsky: My project involves interviewing people who have worked in the field of psychoanalysis, whether as analysts or scholars, and when Phyllis Grosskurth suggested that I contact you it seemed like a great idea. What I think you can bring to bear in a very valuable way is a perspective from outside the discipline, which people in psychoanalysis need to take into account more than they do. I expect you and I will find that we agree about many things, but probably there will be times in our conversation when I show greater sympathy with psychoanalysis than you do. Your two books—the Freud book of 1979¹ and now *Born to Rebel*²—take Freud and Darwin as central figures, and one thing that I want to explore with you is the role of Freud and Darwin in your thinking and how your assessment especially of Freud has changed. Your admiration for Darwin seems to have been consistent, but clearly there has been an evolution in your thinking about Freud in the two decades since you published *Freud, Biologist of the Mind*. I'd also like to hear about how you got interested in Freud and Darwin and how you see your theoretical work connected to your own life experience. So maybe we could start with something autobiographical and then move on to issues of theory and substance.

1. Frank J. Sulloway (1979), *Freud, Biologist of the Mind: Beyond the Psychoanalytic Legend*. New York: Basic Books.

2. Frank J. Sulloway (1996), *Born to Rebel: Birth Order, Family Dynamics and Creative Lives*. New York: Vintage Books.

Frank J. Sulloway: My first academic interest was in Darwin and stemmed from a project that retraced his five-year *Beagle* voyage (1831–1836). At this time I was a junior at Harvard College. I raised about \$30,000 and put together an eight-person film crew that went around South America during the summer of 1968 and made a series of films. Our script was constructed entirely in Darwin's own words. We took his letters, diary, and various published versions of his account of the *Beagle* voyage and strung all these materials together into a self-narrated documentary.

When I came back from this four-month trip, I decided to write my undergraduate thesis on Darwin and the *Beagle* voyage. I was very interested in trying to understand how he had become an evolutionist. After I had completed my undergraduate thesis, I was awarded a traveling fellowship for the following year, and in the course of this fellowship year I did a lot of reading in the field of psychology, and particularly on the subject of creativity. One of the authors I decided to read was Freud. I hadn't previously read any of Freud's works, at least any that I remember. I felt that, if one was to understand how the mind works, Freud was a reasonably good place to start. The first work about Freud that I read was Ernest Jones's biography of him.³ Many ideas that Jones took for granted, such as that there are oral and anal stages to childhood sexuality, struck me as puzzles. As a true believer, Jones would simply say something like, "Then Freud discovered the polymorphously perverse nature of childhood sexuality." The biography didn't go always into the rationale behind the theories.

Having had some training in the history of science, which was the field in which I was about to begin my graduate studies, I was naturally curious about the origins and historical context of Freud's ideas. So I decided to read Freud's correspondence with Wilhelm Fliess. I was immediately struck by the fact that Freud was discussing infantile sexuality in these letters at a time when he was not yet supposed to know about this idea. Well before the self-analysis, there's a letter of December 6, 1896, in which he asserts that the child must be sexual.⁴ A few weeks later Freud connects the child's polymorphous perversity to sexuality in animals, including the sense

of smell.⁵ He also refers to Albert Moll's similar ideas on this subject.⁶ Freud is talking, of course, about the notion of "abandoned erotogenic zones." I knew from my background in the history of biology that behind this general discussion was a well-known biological assumption, namely, Ernst Haeckel's biogenetic law.⁷ Without having to spell it out to Fliess—who, like Freud, had a strong background in biology—Freud was saying that a child *has* to be sexual because the child recapitulates the sexual history of our ancestors.

According to the biogenetic law, the child recapitulates, during the early stages of individual development, the *mature* stages of our ancestors. From our present-day perspective, these mature stages of sexuality can be viewed as "polymorphously perverse"—representing the kinds of erotic interests that dogs have, for example, in feces—erotic interests that are typical of animals generally. The child's form of sexuality is, as Freud put it, a form of "zoophilia."⁸ When I first read these discussions in Freud's letters to Fliess, I thought, "Gee, that's an interesting historical point. Freud appears to have had his first insight into one of his two most celebrated discoveries from the general perspective of late 19th-century evolutionary thought."

PLR: The other discovery being about dreams.

FJS: Yes, the other being his insight into what he believed to be the hidden meaning of dreams. So Freud, at least in his thinking about human development, clearly was borrowing from this great intellectual tradition—namely, 19th-century evolutionary thinking in its special Haeckelian version—and I'd never seen a reference to this influence. I certainly didn't see it in Jones's biography. And since this important insight occurs many months before Freud is supposed to have discovered infantile sexuality through his self-analysis, it didn't appear to me as though he had really discovered infantile sexuality via that route. Or, if he did, he discovered something other than the basic concept.

In any event, none of this evidence linking Freud's thinking about

3. Ernest Jones (1953, 1955, 1957), *The Life and Work of Sigmund Freud*, Vols. I–III. New York: Basic Books.

4. Jeffrey M. Masson, ed. & trans. (1985), *The Complete Letters of Sigmund Freud to Wilhelm Fliess, 1887–1904*. Cambridge, MA: Harvard University Press, p. 212.

5. Letter of January 11, 1897. In Masson, ed. *Freud–Fliess Letters*, p. 223.

6. Letter of November 14, 1897. In Masson, ed. *Freud–Fliess Letters*, p. 279.

7. Ernst Haeckel (1866), *Generelle Morphologie der Organismen: Allgemeine Grundzüge der organischen Formen-Wissenschaft, mechanisch begründet durch die von Charles Darwin reformierte Descendenztheorie*, 2 vols. Berlin: Georg Reimer. See Sulloway, *Freud, Biologist of the Mind*, p. 199.

8. Letter of January 11, 1897. In Masson, ed. *Freud–Fliess Letters*, p. 223.

psychosexual development to evolutionary biology had been discussed by any of Freud's biographers or by other historians of psychoanalysis. I'm sure they had read this section of the correspondence with Fliess, but it apparently meant nothing to them. It meant a lot to me, though, and it clearly meant a lot to Freud, who appealed to similar notions throughout his career and who possessed, of course, a strong background in evolutionary biology. So this particular observation about Freud's intellectual development sucked me in. I thought to myself, "I'll write a paper on this topic." This was in 1972.

At this same time I was taking a graduate seminar at Harvard University with Jerome Kagan, a developmental psychologist of considerable note, and I started a paper on Freud's intellectual development for Kagan's seminar. The more I burrowed into Freud's collected works and correspondence, the more references I kept finding to the idea that ontogeny recapitulates phylogeny. So I started following up these references back to the books Freud was reading and citing in this general context. I consulted Albert Moll⁹ and Richard von Krafft-Ebing,¹⁰ for example, and found that the same kind of evolutionary logic was present in their writings about sexuality. Krafft-Ebing believed that sexual perversions were throwbacks to the forms of sexuality present in our ancestors; so sadism, for example, was believed to be a reflection of the aggression males once used when procuring a mate.¹¹ Krafft-Ebing and others thought that, in humans, these kinds of "atavisms" emerge in pathological cases. This, of course is the same basic logic to which Freud had appealed during his own radical change of thinking in 1896 and 1897, when he went from believing that children are asexual (and hence will be traumatized by premature sexual experiences) to accepting that sexuality in childhood—albeit "polymorphously perverse"—is normal. In other words, Freud had begun to think that childhood sexuality possesses an energy of its own (libido) and that this energy doesn't need to be awakened by stimulation from the outside world. This more dynamic conception of childhood sexuality replaced the theory that Freud soon abandoned when his seduction theory of neurosis finally collapsed in the fall of 1897.

9. Albert Moll (1897), *Untersuchungen über die Libido Sexualis*. Berlin: Fischer's Medizinische Buchhandlung.

10. Richard von Krafft-Ebing (1886), *Psychopathia Sexualis, with Especial Reference to Antipathic Sexual Instinct: A Medico-Forensic Study*, trans. from 10th (1898) German edition by R. J. Rebman. London: Rebman, 1899.

11. Krafft-Ebing, *Psychopathia Sexualis*, pp. 199–200. See Sulloway, *Freud, Biologist of the Mind*, p. 295.

My research project on this aspect of Freud's intellectual development just grew and grew and grew. I took an incomplete in Kagan's course to pursue this historical reconstruction of Freud's ideas. [PLR laughs] My study became a long paper, and then it became a short book. Finally, I decided to do my doctoral dissertation on this topic, and it went on to become a rather long dissertation—eventually over 1,000 typewritten pages. About four years later, when I was finishing this dissertation, I said to Kagan during one of our regular meetings, "You know, I still have an incomplete in your seminar course." Kagan, who had followed all of my developing ideas on this subject, said to me, "I guess you've done enough work now. I'll give you an A." [both laugh]

PLR: You almost had the fate of evolutionary theorist William Hamilton and didn't receive a degree for your work!¹²

FJS: Actually, I was a Harvard Junior Fellow at that time, and during the old days Junior Fellows didn't take degrees. They were evidently considered above that. By the mid-1970s, however, it had become customary for Junior Fellows to take a degree. Anyway, what started as an innocent little paper eventually became my first book. This was an interesting process of intellectual discovery because it lured me in, deeper and deeper. I had no intention of writing a long work on Freud, but I stumbled into a hornet's nest of historical and scientific problems as I tried to make sense out of Freud's intellectual development and his scientific heritage. Because the more traditional analysts who had engaged in Freud scholarship up to that point, including very capable people such as Ernest Jones and Kurt Eissler, did not have a background in either intellectual history or the history of science, they appeared to have missed a lot. This was not generally true of Henri Ellenberger, who missed practically nothing, but Ellenberger's scope in his *Discovery of the Unconscious* was so broad that he wasn't really focused on Freud as a developing individual but rather on Freud's broader place in the history of psychiatry.¹³ So there really was a niche open there.

12. See Sulloway, *Born to Rebel*, pp. 57–59. While a graduate student at the University of London in 1963, William Hamilton proposed a theory of genetically encoded "kin selection" (along with a related notion of "inclusive fitness") as a solution to the longstanding conundrum of why, according to the laws of natural selection, inherently selfish organisms should engage in cooperative behavior. Although it has since proven extremely influential, Hamilton's work was initially deemed insufficient to be awarded the doctoral degree.

13. Henri F. Ellenberger (1970), *The Discovery of the Unconscious: The History and Evolution of Dynamic Psychiatry*. New York: Basic Books.

PLR: You followed a niche-picking strategy.¹⁴

FJS: Mine was definitely a niche-picking strategy. So that's how I got interested in Freud. I must say, having spent seven years on that project, that the more I looked into Freud's ideas, the more disillusioned I became with psychoanalytic theory. Not, however, with Freud as a historical figure. Back then I still thought that psychoanalysis was a terribly interesting theory, even if it was deeply flawed. And I had a lot of admiration for Freud, although it was certainly mixed with criticisms as well. Trained as a historian, I tended to view Freud, including the substantial errors that he appeared to have made, as one might view Aristotle.¹⁵ There's practically nothing in Aristotle's scientific works—in his theoretical biology or his physics—that's valid today, although the observational aspects of his biology have fared better than his cosmology. But a historian writing about Aristotle with the distinct advantage of having a knowledge of 20th-century science wouldn't ridicule him for his stupidity. So I felt that my job was to study the origin of Freud's ideas and not to denigrate Freud for being so stupid as to accept a principle such as the biogenetic law, which is now known to be totally wrong and that was, even during Freud's lifetime, also known to be wrong. This simply wasn't the spirit in which I wrote my book.

When I finally finished the book I did feel, however, that I had invested a great deal of time in understanding the development of a theory that was not particularly useful for solving the problems that had gotten me interested in Freud in the first place. These problems involved scientific creativity and human development more broadly. The more exposure I acquired to academic psychology, the more I realized that almost none of these people were wasting their time with Freud. All I had succeeded in doing in the course of my researches was to show, through historical analysis, why serious behavioral scientists ought no longer to be interested in Freud. So after finishing my book on Freud, I went back to Darwin.

PLR: What was it exactly that you become disillusioned with in the course of writing your book?

FJS: It wasn't so much the methodology as the theories. I subsequently became much more critical of Freud as a hypothesis-tester

14. Sulloway has developed the notion of niche picking as a sibling strategy. See *Born to Rebel*, pp. 95–96.

15. See Sulloway, *Freud, Biologist of the Mind*, p. 500.

and methodologist. But that wasn't the source of my disillusionment in the late 1970s.

PLR: Is this disillusionment reflected in the book?

FJS: Yes, there's a certain ambivalence purposely reflected in the book. I was trying to be balanced. I didn't want to pull a Jeffrey Masson, who became disillusioned with Freud's theories and then concocted a strange historical scenario in which Freud goes astray through intellectual cowardice (a preposterous thesis if there ever was one).

PLR: You were before Masson though. [laughs] *The Assault on Truth* was not published until 1984.¹⁶

FJS: Well, yes, but I had met Masson and knew something of his opinions about Freud, and I did not want to pull a Jeffrey Masson-like intellectual stunt. In other words, I didn't want to be a sensationalist, especially at the expense of historical balance. I was trying to give Freud his due and at same time to make the argument that almost every major assumption in the psychosexual theory of development that underlies the rest of his psychoanalytic theorizing—in the interpretation of dreams and so on—is based on outmoded 19th-century assumptions that are now known to be dead as a doornail.

PLR: The sense that Freud's thinking is based on outmoded concepts doesn't come through to me in *Freud, Biologist of the Mind*. The death instinct is described as logically coherent and justified rather than being attacked in your book.¹⁷

FJS: The book takes for granted—perhaps too much so in some places—that the educated reader is aware that such notions as the inheritance of acquired characteristics (which is a necessary prerequisite for the biogenetic law), and the biogenetic law itself, are now known to be totally wrong ideas. Actually, in several places I am quite explicit on this important point.¹⁸ On this subject one doesn't have to beat a dead horse, or at least that was what I thought at the time. Once I had shown, for example, that the death instinct possesses a peculiar biogenetic logic, which has to do with regression to past life forms and so on, I had really made my point. Such a theory, no matter how conceptually elegant, couldn't possibly be true.

16. Jeffrey Moussaieff Masson (1984), *The Assault on Truth: Freud's Suppression of the Seduction Theory*. New York: Farrar, Strauss & Giroux.

17. Sulloway, *Freud, Biologist of the Mind*, pp. 395, 409.

18. Sulloway, *Freud, Biologist of the Mind*, pp. 439–442, 497–498.

PLR: I agree with you completely about the untenability of Freud's Lamarckianism. But it seems to me that the discussion in your book sometimes conflated Darwinian and Lamarckian theories of evolution. You showed Freud's participation in a scientific tradition, but you didn't make a concerted effort to differentiate between the parts of his thought (if any) that might be considered Darwinian—and therefore soundly based—from those based on untenable concepts. You were concerned with Freud as a “biologist of the mind” in a broad sense. You used the relationship to Fliess to demonstrate that Freud was influenced by a set of contemporary assumptions that explained his thinking in another way but didn't necessarily undercut it.

FJS: The critique of Freud is definitely there in the book. It's perhaps not as developed as I would make it if I were rewriting the book now. In the last chapter, I'm quite clear about the fact that there are fundamental defects in the whole theory of psychosexual development and in the hydraulic theory, which are explained by Freud's unfortunate reliance on outmoded assumptions drawn from 19th-century biology. I go on to ask: What, then, can we give Freud credit for? I gave him credit for tackling the most interesting psychobiological topics of the day and for identifying them as fruitful, but not for solving them. I made my allusion to Aristotle and other great thinkers in this connection. Most of these philosophers and scientists, working within the conceptual limitations of their day, were wrong on a lot of issues, but we still give them their due as great thinkers.

Back in the late 1970s I tended to think of Freud as someone who deserved credit for having a brilliant mind and an ability, given the assumptions of the day, to spell out exactly where these assumptions would lead a logical thinker, even though it turns out that most of these assumptions were wrong and led Freud in the wrong direction. There are always a lot of people around in any given historical period who don't ever grasp the big picture. They don't know how to draw all the available information together. Then someone comes along, like Freud, who reads voraciously and synthesizes all the existing literature. Suddenly this one person has a very sharp picture and creates a coherent theory, which now becomes a target for potential refutations. Then this new theory is either confirmed or not. That's the stage at which I left Freud in my book.

The great tragedy of psychoanalysis is that, having staked out fertile territory and having proposed, through Freud's own concepts, an extraordinary theory that united it all, its practitioners then failed to do what is normal and indeed essential in science. The second step of science after one has developed a hypothesis is to test that hypoth-

esis. Freud and his followers generally failed to take that second step, and this failure proved fatal to the whole enterprise.

PLR: I'm prepared to agree with you about a lot of this. Perhaps the project needs not to be abandoned but to be grounded more securely through the type of testing you're talking about.

FJS: That's an interesting issue. Almost no one, in my opinion, is effectively doing that, mainly because the discipline does not train its practitioners to do formal research, and the people in medicine who have been adequately trained in scientific methodology are too busy doing their own research to do what psychoanalysts should be doing for themselves.

PLR: Psychoanalytic theory isn't monolithic. It may be necessary to discard many components in order to regroup it. To anticipate something we might talk about later: Bowlby is someone I know you admire greatly and whom I also admire very much. In my view, he came as close as anyone to laying a foundation for contemporary psychoanalysis.

FJS: There are two crucial issues relevant to the intellectual fate of psychoanalysis—both past and present. One is the nature of the theory, which was badly flawed from the start. The other is the nature of the personnel who are attracted to the discipline and who then, through the form of education that it offers, fail to learn the basic methods of science. Even the most faulty of theories eventually is corrected or discarded as long as a discipline's practitioners engage in hypothesis testing. Psychoanalysis did not do this, and, as a result, the field has become mostly a dead end for research. Bowlby was an exception, not because he stayed in psychoanalysis but because he effectively left psychoanalysis. He clawed his way to the real world of modern science, which is an extraordinary thing for someone to have done given where Bowlby started out. I once asked him how he did it, and he told me that he owed a great debt in this connection to his Cambridge University education in natural science.

PLR: I don't think it's fair to say that he left psychoanalysis.

FJS: For all practical purposes he was ostracized from the psychoanalytic movement in England. His psychoanalytic colleagues had almost zero interest in his work, and his closest colleagues, such as Mary Ainsworth, eventually became academic psychologists.

PLR: I've interviewed Mary Ainsworth for this book.

FJS: I'd like to read that interview. I'm not saying, by the way, that

Bowlby didn't still feel some intellectual continuity between what he was doing later in his life and the field where he started out.

PLR: He never renounced his identity as a psychoanalyst.

FJS: True enough, but we must keep in mind that there are two key factors that drive any theory forward. One is the theory proper and the other is the methodology—the testing. At the level of methodology, Bowlby effectively moved into the world of science.

PLR: But he retained close ties with John Sutherland and others in the object relations tradition of psychoanalysis. When you refer to Bowlby, you invariably call him a psychiatrist or a psychologist, never a psychoanalyst. I agree that the cold shoulder given Bowlby was extremely unfortunate. I would simply say that, insofar as the project of grounding psychoanalysis in something compatible with a scientific perspective is tenable, which I hope it is, Bowlby is a crucial figure in pointing the way to that.

FJS: Well, I heartily agree. But it's also fair to say that the people who are the heirs to this tradition—the psychologists who work on attachment behavior—have mostly done their research through academic channels.

PLR: I think there's been a healthy cross-fertilization.

FJS: The main problem, as I have said before, is that psychoanalysts don't get any real training in science. So they don't know how to do hypothesis testing, experiments, or epidemiological studies. They know how to read a book and how to pick up on an interesting theory or idea and either be convinced or not be convinced of it. But the cutting edge of research in behavioral science areas like those in which Bowlby was interested is now found in work done by academic psychologists, not psychoanalysts. These academic psychologists are not interested in rescuing psychoanalysis because, in general, they don't think there's anything worth rescuing.

PLR: I can say that Mary Ainsworth has a different perspective on psychoanalysis. She is quite well disposed toward it.

FJS: What about Mary Main? She's a mainstream academic.

PLR: Stephen Mitchell cited her work very favorably in talking to me, but I don't know what she would say about psychoanalysis. It would be interesting to find out.¹⁹

19. Mary Main's views on psychoanalysis are contained in "Discourse, Prediction, and Recent Studies in Attachment: Implications for Psychoanalysis."

FJS: Yes.

PLR: In a passage from *Freud, Biologist of the Mind*, you say that "Freud established perhaps the most comprehensive evolutionary explanation of the origins of human behavior that has yet been formulated in science."²⁰ That's high praise indeed. I know that it reflects a certain stage in your thinking, but I picked it out to suggest that the critique of the Lamarckian and Haeckelian dimensions of Freud's biology wasn't something that you pressed in that book.

FJS: Well, it depends on which parts of the book one reads. Moreover, the sentence you have quoted is not inconsistent with my conclusion that this grand evolutionary vision was basically flawed in fundamental ways. It's also true, however, that my main intent in the book was not to conclude whether Freud was right or wrong but rather to illuminate his intellectual development. Probably the main reason I didn't emphasize the issue of validity more is that my training was as an intellectual historian, and one of the most basic principles that gets drummed into students in this field is "Don't commit the fallacy of Whiggish history." Whiggish history is the end result of seeing events and ideas as leading up to the present, which is considered "correct." Everything that isn't on this path to truth, virtue, and justice is considered wrong.

In Theodore Shapiro & Robert N. Emde, eds. (1995), *Research in Psychoanalysis: Process, Development, Outcome*. Madison, CT: International Universities Press, pp. 209–244. Main's chapter, which traces the progression from Bowlby's ethological-evolutionary theory of attachment, to Ainsworth's Strange Situation procedure, to Main's own Adult Attachment Interview, shows Main to be fundamentally sympathetic to psychoanalysis. Main notes that "we have the capacity for attending to, processing, and drawing inferences from input that does not reach the usual levels of awareness," and this "has implications for the development of defensive processes and is in keeping with analytic theory" (p. 214). She cites empirical evidence that a given parent's response to the Adult Attachment Interview—responses classified by experimenters as secure, dismissing, preoccupied, or disorganized—"strongly resembles the infant's response to the [same] parent in the Strange Situation" (p. 225)—responses that are classified independently as secure, avoidant, ambivalent/resistant, or disorganized. Main compares an insecure parent's behavior toward his or her infant to that of a patient who, according to Freud, "does not remember anything of what he has forgotten or repressed, but acts it out" (p. 255); and she argues that these "dyadic adjustments begin in the earliest weeks of infant life" (p. 236). I am grateful to Frank Sulloway for the reference to Mary Main's paper.

20. Sulloway, *Freud, Biologist of the Mind*, p. 367.

PLR: But if you're a historian of science, isn't such a perspective to some extent unavoidable?

FJS: Of course, which is why I tried to make my views crystal clear in several key places. For example, I stated that "various unconfirmed aspects of Freud's theories may be correlated almost point by point with his erroneous premises" and that "acceptance of Freud's historical debt to biology requires a rather uncongenial conclusion for most psychoanalytic practitioners, namely, that Freud's theories reflect the faulty logic of outmoded 19th-century biological assumptions."²¹ In the last half century, however, the history of science was influenced greatly by an attempt to get away from this Whiggish view and instead to consider erroneous beliefs as being just as worthy of study, understanding, and even empathy as those beliefs that have triumphed over them. This is the kind of relativistic perspective that has been in the ascendancy, culminating in the view among some historians of science that erroneous theories must be judged exactly the same way as theories that have withstood the test of time.

PLR: But you don't buy that, do you?

FJS: Oh no, I don't. But I *was* influenced enough by the general principle of "Don't commit the Whiggish fallacy" that I bent over backwards not to pounce on every mistake Freud made and then turn around and declare the guy was clearly an idiot. I was trying to reconstruct the intellectual context that made Freud's ideas seem plausible or reasonable to him and his contemporaries, even if those ideas are now known to be incorrect. If such ideas were reasonable at the time, then it was legitimate for Freud to propose and develop them. Now, there comes a point at which every true scientist, after proposing new hypotheses, has a responsibility to take the next step in science. This is something that Patricia Kitcher has written about in a very interesting book.²² The gist of her argument is that Freud drew in very eclectic ways from a variety of disciplines, but that, as these disciplines revised their understanding of the mind via hypothesis testing, Freud refused to change his own. That, in a nutshell, was the fundamental flaw of psychoanalysis.

PLR: I would agree with a lot of that. Did it make any difference for your work that the Fliess letters were published only in abridged form at the time you were writing your book?

21. Sulloway, *Freud, Biologist of the Mind*, pp. 497, 499.

22. Patricia Kitcher (1992), *Freud's Dream: A Complete Interdisciplinary Science of Mind*. Cambridge, MA: MIT Press.

FJS: Most of the really nifty stuff in the Fliess letters that wasn't published in 1950, in the original expurgated edition, was alluded to either by Max Schur²³ or by Ernest Jones²⁴ in their biographies of Freud. When the unexpurgated edition came out, I was actually surprised how much of the excised material I had known about either by reading Schur or Jones or by reading Fliess himself, and seeing what the logic of Freud's thinking was, as well as the empirical support that he eagerly provided for this logic.

PLR: Certainly the Emma Eckstein episode was known through Schur's work.²⁵

FJS: There were also many allusions in Jones and Schur to Freud's efforts to develop a theory of psychosexual stages in terms of Fliess's two biological periodicities (23 and 28 days) and to use the temporal crisscrosses between these two cycles as a mechanism for determining when a repression or a fixation would be most likely to occur. What's important about the intellectual collaboration between Freud and Fliess is not the specifics of the theories that Freud was playing around with, which can be viewed, superficially, as ridiculously silly and wrong at one level—and so have been generally dismissed by historians of psychoanalysis—but rather the broader set of underlying biological assumptions that goes with these theories. The most important of these assumptions, or, more precisely, the most important *consequence* of these assumptions, is the idea that children are spontaneously sexual because their development is guided by a sexual biology that drives conception, gestation, growth, and even death. If that were true, then a proper understanding of human sexual development required a biological paradigm to go with it. From their perspective, the details of what Freud and Fliess were jointly working on might have been wrong, but the gist of the paradigm underlying those details had to be right. This intellectual collaboration was taking place at a time when Freud's thinking was moving from an almost complete reliance on the environment (trauma) as a cause of

23. Max Schur (1972), *Freud: Living and Dying*. New York: International Universities Press.

24. Jones, *The Life and Work of Sigmund Freud*.

25. Max Schur (1966), "Some Additional 'Day Residues' of 'The Specimen Dream of Psychoanalysis.'" In *Psychoanalysis: A General Psychology: Essays in Honor of Heinz Hartmann*, ed. Rudolph M. Loewenstein, Lottie M. Newman, Max Schur & Albert J. Solnit. New York: International Universities Press, pp. 45–85. See also Schur, *Freud: Living and Dying*.

neuroses to a theory of the child's being a sexually active source of its own neurosis. This same basic concept was at the heart of Fliess's own theory of human development, because he believed that the periodic ebb and flow of sexual chemistry causes the organism to develop the way that it does. The best evidence for this theory, according to Fliess, was that spurts in growth occur at even multiples of 23 and 28 days and are accompanied by manifestations of the sexual chemistry that propels them.

PLR: You saw a lot of Freud in Fliess.

FJS: They saw a lot of themselves in each other. All I did was to show that this was the case in spite of the repeated denials of analysts-turned-historians who were saying that Fliess was only a sounding board for Freud and nothing more. It was certainly ironic and an eye-opener to go back to Fliess's published works and find discussions of infantile sexuality in a book that came out almost a year before Freud's self-analysis.

PLR: Wasn't Fliess's book on the nose published in 1897?²⁶

FJS: Freud read it in manuscript even before, in 1896. The book discusses thumb-sucking as a form of sexual pleasure that intensifies on days when biorhythms reach their peaks. Various aspects of bowel movements were also sexual, according to Fliess. That set of Fliessian ideas is basically the starting point for Freud's later theory of psychosexual stages. These discussions seem a bit strange unless one understands them in terms of the biogenetic law. This was the basic theory of sexual evolution held by many biologists and some psychiatrists at this time. Sexual evolution, according to Darwin's German proselytizer Ernst Haeckel, developed from a single-celled organism that invaginated to form a mouth—the oral stage of sexuality. After the evolution of a gastrointestinal tract, more sophisticated genitalia emerged, which were associated with the anal region. Eventually there evolved a phallic stage. That's the general theory that Freud and Fliess were playing around with, and that's why they both attached sexual significance to the mouth, the alimentary canal, the anus, and so forth. Unless one understands this important historical point, one cannot understand either their 17-year scientific relationship or the development of Freud's own ideas.

26. Wilhelm Fliess (1897), *Die Beziehung zwischen Nase und weiblichen Geschlechtsorganen: In ihrer biologischen Bedeutung dargestellt*. Leipzig: Franz Deuticke.

PLR: When you became interested in Freud, was there any sense that you were trying to understand things about yourself? Did you use the study of psychoanalysis in a way that could be called self-analytic?

FJS: I was not motivated to study Freud for that reason. When I read *The Interpretation of Dreams* I initially played around with self-analysis, as any reader would, and I analyzed some of my own dreams. It was fun to do. But this wasn't a primary motive for my study of Freud's ideas. I certainly didn't turn to Freud because I felt, "Gee, I really want to come to grips with my own life." Still, one couldn't read Freud and not try him on, so to speak, just to see how the theory worked.

PLR: Did you find any connections between yourself and what you read in Freud? Were there certain moments when you felt that what you saw in his work was true for you?

FJS: Basically, no. I'm sure there were some insights that I had about myself that I probably got by reading Freud. But they weren't unique to my having read Freud. They were probably general insights that I might well have had if I had been doing a lot of in-depth reading in developmental psychology. I didn't say to myself, "Oh, my God, I've repressed the memory of blah-dee-blah. No wonder I've had this paralyzed arm all my life, and now it's gone away." [laughs] There wasn't anything like that.

PLR: It never really connected with you in a personal way.

FJS: After the initial period of experimentation, during which I did make some connections, I don't really think so. At a more general level, however, I thought the theory in all its complexities was fascinating. I sometimes used to think, "If Lamarckian inheritance and the biogenetic law had been true, this theory might very well have been predominantly true." It has a logical beauty and coherence to it, as long as those biological principles are allowed to operate and one can appeal to inherited memories. The theory offers, in a more psychologically palatable form, the kind of biologizing of the human organism that evolutionary psychology and behavioral genetics have offered since the 1970s and 1980s. But that's a much more complicated set of theories to get into. Freud did it all at a much simpler level. Essentially, the phylogenetic past became life writ large today. As Freud once commented about the biogenetic inevitability entailed in his theory of psychosexual stages: "In all this the phylogenetic foundation has so much the upper hand over personal accidental

experience that it makes no difference whether a child has ever really sucked at the breast or has been brought up on the bottle and never enjoyed the tenderness of a mother's care. In both cases the child's development takes the same path."²⁷

PLR: I don't think that many people who become excited on reading *The Interpretation of Dreams* are relying on the biogenetic law as an intermediate link between what they see in Freud and what they find in themselves.

FJS: No, one certainly doesn't have to. But, as Freud well knew, there comes a point when every orthodox Freudian interpretation is subsumed by his theory of psychosexual development, and at *that* key point the ultimate meaning of a dream, slip of the pen, or neurotic symptom is largely dictated, or at least significantly constrained, by these biogenetic assumptions. People who read Freud's works, including *The Interpretation of Dreams*, and who do not understand these underlying biological assumptions really do not understand Freud's theories as he intended them to be understood. These readers are mistaking a "pure psychology," as Jones and others (including Freud himself) have sometimes tried to characterize psychoanalysis, for a distinctly psychobiological set of theories.

PLR: In my own thinking about Freud, the libido theory is not so crucial. There are other dimensions that are more productive and fruitful. You credit Freud with being able "to recognize the truly dynamic and infantile source of man's unconscious."²⁸ That doesn't necessarily involve sexuality, but it does assume the lifelong influence of early experience. You refer elsewhere to Darwin's having said that the first three years of life are the most important, and you link that claim to Freud's belief in the "pertinacity of early impressions."²⁹ I think one can put aside the debate about what is sexual and ask simply whether the role of early childhood experience is crucial and occurs below the level of consciousness, as Freud thought—and as Darwin seems to have thought as well. Does that seem plausible to you?

27. Sigmund Freud (1940), *An Outline of Psycho-Analysis. Standard Edition*, 23:188–189. London: Hogarth Press, 1964.

28. Sulloway, *Freud, Biologist of the Mind*, p. 128.

29. Frank J. Sulloway (1991), "Darwinian Psychobiography." Review of *Charles Darwin: A New Life* by John Bowlby. *New York Review of Books*, October 10, p. 29.

FJS: Let's make it absolutely clear that if people reject Freud's sexual theory, they are not Freudians. Freud himself made this clear in his disputes with followers who broke away from him after 1908 or so. To Freud, psychoanalysis was not just a belief in the unconscious and the power of early experiences. These were ideas that many of his contemporaries also endorsed. There are some comments in my book, such as the one you just read about the dynamic unconscious, where I was writing, as a historian, of Freud's own thinking about what he was doing. I was not necessarily endorsing something that I consider a valid Freudian discovery. For example, I might say something like, "Freud had an insight into the polymorphously perverse nature of infantile sexuality." This doesn't necessarily mean that I believe what Freud believed. I'm simply saying that Freud in his own mind had an "insight."

I didn't realize until after I had published my book on Freud that a reader might infer from such passages that I was endorsing Freud. It was only when Fred Crews wrote his article "Beyond Sulloway's *Freud*" and picked up on almost every one of those phrases that I finally became aware of it.³⁰ When I might be writing, for example, about Freud's remarks in a letter saying "Here I discovered the meaning of dreams," I took it for granted that this is what *Freud* thought he had discovered, not necessarily some fundamental truth that I was also endorsing. There are some forms of language in my book that Crews took to mean that I bought hook, line, and sinker that Freud had really discovered a bunch of things that were real. But I generally meant these statements to be understood in a more metaphorical way, without necessarily implying that I was passing scientific judgment on Freud's ideas. With hindsight, I think that some of this phraseology was simply careless, and I now wish that I had been clearer on this issue.

PLR: I think it's admirable that you are able to look back at something that was, after all, published 20 years ago and see Freud differently from the way you did then. It wouldn't be healthy if you wanted to affirm every word. For my own part, I think it's important to distinguish between being a Freudian—which I'm not—and adherence to a psychoanalytic mode of thinking that I believe to be intellectually sound. I'm simply trying to clarify what you were saying then and how you see things now.

30. Frederick Crews (1986), "Beyond Sulloway's *Freud: Psychoanalysis Minus the Myth of the Hero*." In *Skeptical Engagements*. New York: Oxford University Press, pp. 88–111.

FJS: If I were to revise this book, there are places where I would definitely want to modify my language to clarify that I am not necessarily endorsing the validity of psychoanalysis. In most instances, I was simply trying to recreate how Freud felt about what he was doing.

PLR: Certainly, when you use the word “recognize” you imply that he was discovering something that was really there. And you do say that he recognized “the *truly* dynamic and infantile source of man’s unconscious.” In conjunction with the encomiastic statement I cited earlier about Freud’s having established “the most comprehensive evolutionary explanation of the origins of human behavior” or your quotation from Philip Rieff that Freud produced “the most important body of thought committed to paper in the 20th century,”³¹ I think all that conveys strongly that you agreed with much of what Freud had said and were trying to show its logical coherence.

FJS: There’s no question that I had an enormous admiration for Freud as a grand theoretical synthesizer, as generally conveyed in the comments you are referring to. I wouldn’t change in a major way my views on that subject. But I would, if I were revising my book, alter some of those discussions that appear to be endorsing the specifics of the theory. What I did endorse, as I stated explicitly in the final chapter of my book, was that Freud awakened interest in many important areas of research, including infancy, childhood, irrational behavior, and so forth. I did not give him high marks, however, for his particular theories on these subjects. Again, if I were revising my book today, I would try to make a sharper distinction between Freud the theorist, seen in the context of his times, and the theories themselves.

PLR: I was struck by Darwin’s seeming to agree with Freud about the importance of the first three years of life. So if we’re going to reject Freud on this, we’re also going to have to reject Darwin.

FJS: That’s easy. Darwin wasn’t an expert on this topic. [laughs]

PLR: Maybe both of them were wrong. It seems there are three major components of Freud’s assertion. One is that there is such a thing as the unconscious part of our minds. Second, that the unconscious has its roots in infantile experience. The last is that the unconscious is dynamic. If we take those three claims separately, I wonder whether in your current thinking as a historian of science, as a social scientist more generally, or simply as a human being, you would agree with any or all of them.

31. Sulloway, *Freud, Biologist of the Mind*, p. 358.

FJS: I certainly believe that there are unconscious aspects to thought, but I do not believe in Freud’s dynamic unconscious as a kind of sealed-off area of the mind in which a whole other psychic life, dating mostly from early childhood, is imprisoned by the powers of repression. That theory, and the psychobiological mechanisms that Freud used to justify it, are deeply flawed by the outmoded biology that went with them. I don’t see any way to rescue Freud’s theory of the unconscious.

PLR: But you accept the existence of unconscious phenomena?

FJS: There probably isn’t a living psychologist worth his grit who doesn’t accept that there are “unconscious” mental phenomena.

PLR: So that much is agreed. Are we talking about something that is not voluntarily accessible to consciousness?

FJS: Not exactly. I’m thinking more of the subtle things one encounters, for example, in tachistoscopic experiments which show clearly that the mind can perceive something communicated as a subliminal message flashed quickly across the retina but that it doesn’t recognize consciously. This is not the same thing as there being an unconscious (repressed) idea to which the mind has no direct access. It’s just that there are threshold effects in the perception of information within the brain. Information is processed at some levels that are inaccessible to consciousness and at other levels that *are* accessible. But none of this bears on Freud’s much more controversial claim that the unconscious is formed by pathological repression. The closest I would come to agreeing with Freud’s concept of pathological repression has to do with people whose minds exhibit “dissociation” of a pathological sort. I am not a psychiatrist, however, and I do not really know how to integrate that kind of medical evidence with Freud’s far more grandiose claims about repression sealing off a portion of the unconscious mind in normal individuals. In general, I think Freud’s argument that the bulk of the interpretable meaning of adult behavior is closely linked with this sealed-off world of the dynamic unconscious is untenable.

PLR: I’m not sure that we need to say “sealed-off world of the dynamic unconscious.”

FJS: I’m just repeating *Freud’s* own way of talking about it—the theory as he understood it after 1900 or so. Maybe a psychoanalyst today would express this idea differently, but then this person would not be endorsing Freud’s own model of the mind or his related theory of why the mind develops neurotic symptoms. In any event, I don’t think

there's anything going on in the mind like Freud's own notion of the dynamic unconscious. Moreover, I certainly don't think such a hypothetical mechanism is the key to understanding dreams, slips of the pen, and so forth, although some slips of the pen do appear to involve the more innocent and lackluster form of "unconscious" thinking that is widely accepted in psychology. So there's a whole range of levels at which one can talk about unconscious phenomena, some of which are compatible with current psychology and some of which are not. Unfortunately for Freud, the ones that really count in his theory of psychopathology, as well as in his theory of human development, are the ones that are incompatible with the modern scientific understanding of the brain.

PLR: I suppose the fundamental question for me is whether there is some part of our minds that remains forever mysterious to ourselves. As Jonathan Lear has argued, "There is something which would count as a global refutation of psychoanalysis: if people always and everywhere acted in rational and transparently explicable ways."³² I'm thinking in this connection of your excellent piece in the *New York Review of Books* about Bowlby's biography of Darwin, the gist of which you incorporated into *Born to Rebel*.³³ Again, I'm trying to use Bowlby as a way of rescuing or preserving what I think is valuable in psychoanalysis. I want to see whether we don't share more common ground than one might imagine at first glance.

FJS: Let me just say something at the outset about Bowlby's treatment of Darwin and my own endorsement of it. Much of what works in that treatment of Darwin's life has nothing to do with the first three years of life or with the Freudian unconscious. It has to do with the entire pattern of Darwin's childhood relationships with his sisters, and his father, and the mother whom he lost when he was eight. That's what makes Bowlby's psychological explanation far more plausible than Freud's various attempts at psychobiography. Bowlby was trying to get away from the notion that everything of psychological importance takes place during the first three years of life and through the operation of repression in whatever form—primary (organic) or secondary (psychological). His explanation of Darwin's illness as a form of panic anxiety does not require him to

32. Jonathan Lear (1998), "On Killing Freud (Again)." In *Open Minded: Working Out the Logic of the Soul*. Cambridge, MA: Harvard University Press, p. 25.

33. Sulloway, "Darwinian Psychobiography," pp. 28–32; and *Born to Rebel*, pp. 140–145.

endorse the notion that there is a whole sealed-up portion of the psyche that motivated Darwin to think and act without even being aware of the reasons why.

PLR: The question of the importance of the first three years is different from the question of repression.

FJS: Well, they're two issues that, in Freud's mind at least, went together. The close linkage of these two ideas was demanded, by the way, by Freud's biogenetic conception of human psychosexual development, which led him to claim that all secondary (and psychological) repressions are preceded by primary and organic (or biological) repressions occurring during the first three years of life. In any event, these two ideas are largely irrelevant in Bowlby's own approach to Darwin, which entails looking at the entire period of Darwin's youth and at his family to see the characteristic ways in which he related to his parents and siblings and asking whether these family relationships had a lasting influence on his character and later on his science.

And the answer to these questions appears to be manifestly yes. This conclusion isn't so much psychoanalysis as it is simply good, sensitive psychobiography in which Bowlby considered the entire panoply of childhood experiences as a continuous and collective source of developmental trends. So, in Bowlby's theoretical account, Darwin could have experienced the life that he did in his own family up to the age of three and then been adopted by another family. In that case he would probably have turned out to be a rather different person, though not dramatically different, because we know from behavioral genetics that about 40% of the variance in personality is heritable. But many of Darwin's characteristic peculiarities—such as his extraordinary deference to his sisters and his feeling that he was in constant need of tutoring by them—were behavioral patterns that he seems to have developed to a significant degree through interaction with his sisters.

PLR: Again, I don't think there's a great difference between our views here. I'm asking with genuine curiosity, how important are the early years? I'm taking off from your statement that Freud recognized the "dynamic and infantile sources of the unconscious."

FJS: I certainly wouldn't make that statement today, except on Freud's behalf to summarize the gist of his theory, which was how I meant it to be taken.

PLR: Let's consider the question of repression. In the Bowlby review,

you connect Darwin's statement about his sister, "But what will she blame me for now?" I made myself *dogged* so as not to make myself care what she might say," with his beloved phrase to describe genius, "It's *dogged* as does it"; and you go on to suggest that "from Bowlby's perspective these valuable qualities of Darwin as a scientist can be seen as part of his response to his intimidating father and overzealous older sisters."³⁴ It seems to me that the surfacing of the word *dogged* in two different contexts is something that a psychoanalyst would say confirmed his sense of how life works. A pattern of childhood experience—even if it was set at the age of eight rather than the age of three—shaped Darwin's character in a way that had a life-long effect. The word then can be called a signifier or symptom that came to his mind for reasons that are determined unconsciously in a truly revealing way.

FJS: I'm not sure I agree with your comment about this particular thought process being determined unconsciously. More important, I don't view the material you've quoted from my Bowlby review as being particularly psychoanalytic or even "unconscious." That's just good biography. It involves tracing characteristic patterns of behaving within the family and seeing whether they have a broader manifestation in someone's adult life. In my review, I added pertinent examples that Bowlby hadn't actually used, such as the phrase "it's *dogged* as does it" as Darwin's definition of genius. In the second half of the review I made a concerted attempt to show that Bowlby didn't know as much about Darwin's life as he perhaps would have liked, and one can actually make a stronger case for his psychobiographical argument than he himself was able to do. My review was therefore a critique of Bowlby's biography in the most complimentary sense. In so doing, I threw in some additional biographical evidence, and I thought his arguments and my own were all pretty commonsensical. This additional evidence drew on Bowlby's ideas about the importance of attachment relationships, but there was nothing deeply "repressed" or "unconscious" about the psychological mechanisms posited by this argument. This kind of argumentation goes on throughout most of Bowlby's biography and in my own analysis at a level that Darwin himself would have appreciated and understood, had we probed him on it. Now, there *are* a few interesting "psychoanalytic" issues such as the wonderful story about Darwin's playing a word game very much like Scrabble where somebody added the letter "m" in front of the word "other" to form the word "mother."

34. Sulloway, "Darwinian Psychobiography," p. 31.

PLR: Exactly.

FJS: And Darwin, the story goes, stared at the word and repeatedly said: "MOE-THER; MOE-THER; there's no word 'MOE-THER.'"³⁵ Unfortunately for psychoanalysis, that's not necessarily evidence of the operation of Freud's dynamic unconscious.

PLR: Why not?

FJS: Because people don't have easy access to Freud's dynamic unconscious, and we all have little mental blocks here and there that have nothing to do with pathological "repression." If someone can point out the connection between a mental lapse and an "unconscious" idea to the person who exhibits the lapse and this person instantly, as in Darwin's case, recognizes the connection, the motive for this lapse is not repressed in the Freudian sense.

PLR: The point is not whether Darwin was able to recognize the lapse when it was brought to his attention, but that he committed it in the first place. When someone told Darwin, "But that's *mother*," he must have said, "Oh my goodness, now I see that it's the word."

FJS: But there's nothing repressed about that realization.

PLR: Surely his inability to recognize the word in the first place is evidence of repression.

FJS: Not necessarily. Just to be very clear, my point is that Darwin's lapse is not necessarily "repression" in the sense that Freud would have understood it. It's clearly evidence of a mental block or a dissociation, and we can use some word like repression to describe it, but the lapse has nothing in common with the kind of organic pathological repression associated with everything present in Freud's dynamic unconscious. With the *dynamic* unconscious that lies at the heart of Freud's theory of the neuroses and his theory of psychosex-

35. Gwen Raverat (1952). *Period Piece*. New York: Norton, 1953, p. 245. Raverat, Darwin's granddaughter, facetiously comments: "I feel that the Psychologists might get a great deal of fun out of this anecdote—I beg their pardons, I don't mean fun, but Important Information; clues to the conception of the *Origin of Species*, on the one hand, or to his ill health on the other; both of which developments could doubtless be proved by this story to be the direct consequences of the early death of his own MOETHER." Raverat does not expressly state that "mother" was formed by adding "m" to "other," but this seems the most likely explanation. Her spelling of "MOE-THER" presumably reflects Darwin's own misprision that caused him to stumble over the word. I am grateful to Frank Sulloway for this reference.

ual development, a person almost never attains direct awareness of the motive lying behind a mental lapse or slip. All that a person can be consciously aware of are the indirect repercussions of such a repression, through dreams, symptoms, “resistance,” and so on. Therapists almost never encounter an instance where they say directly to a patient, “Actually, because *this* happened when you were a year and a half, you’re a repressed homosexual.” And the patient responds, “Right on. Now I see the light.” That simply doesn’t happen with Freud’s dynamic unconscious.

PLR: But that is a caricature. There are plenty of examples in *The Psychopathology of Everyday Life* where the meaning of a slip becomes clear to the subject as soon as it is pointed out, but the motive is still held by Freud to be a repressed sexual or aggressive wish. Your supposition about what doesn’t happen in therapy may divert our focus from what was happening in Darwin. Darwin’s “mental block,” as you call it, that prevented him from recognizing the word mother is precisely a symptom that provides indirect evidence for the workings of repression.

FJS: Darwin didn’t require years of psychotherapy to appreciate that he had a mental block concerning the memory of his mother. He discussed this point in his *Autobiography* and gave a plausible explanation for his block, namely, the refusal of his sisters, out of grief, to mention his mother’s name after she had died.³⁶

PLR: The fact that there was a conspiracy of silence within the family concerning this traumatic event helps to explain why it should have precipitated a repression. That’s perfectly consistent with a psychoanalytic view. The death of his mother was obviously emotionally charged and the subject of intense conflicts, both in the family and in Darwin’s own mind.

FJS: I’m simply trying to say that Bowlby’s account is not a Freudian account.

PLR: Can we call it a psychoanalytic account?

FJS: I don’t even think it’s particularly psychoanalytic. Let’s consider the work of somebody like Mary Main, who is a second-generation attachment theorist with no training in psychoanalysis that I know of. She might easily have read a Darwin biography and written an

36. Charles Darwin (1876), *The Autobiography of Charles Darwin, 1809–1882. With the Original Omissions Restored*, ed. Nora Barlow. London: Collins, 1958, p. 22.

article giving the same general account of Darwin’s intellectual and emotional development as Bowlby provided and that has no formal relationship to psychoanalysis other than having borrowed and further developed the concept of attachment through Bowlby. This kind of explanation is not psychoanalytic; rather, it’s psychobiographical. That’s the common ground between the two approaches. In any event, psychoanalysts almost universally rejected Bowlby’s ideas about attachment, in the 1950s and 1960s, as being antipsychoanalytic and anti-Freudian.

PLR: There’s no doubt that Bowlby’s ideas initially aroused a lot of opposition among his colleagues. After all, he challenged both the one-sided emphasis on fantasy of Melanie Klein and the libido theory of Freud. But that doesn’t make him antipsychoanalytic. In particular, he retained close intellectual and personal ties with the Independent tradition of psychoanalysis, whose best-known representatives include Winnicott and Michael Balint. As Bowlby himself wrote, “Historically attachment theory was developed as a variant of object-relations theory.”³⁷ I don’t want to engage in speculation about what Mary Main might or might not have done.

FJS: Fair enough, but I’m just using that hypothetical example to illustrate my general point.

PLR: To me the concept of the dynamic unconscious has a lot of validity to it. Even though I’m prepared to cast aside the libido theory and even to be skeptical about the role of the very earliest years of experience in shaping how our minds work, this example of Darwin’s not recognizing the word mother is important to me in showing that the dynamic unconscious plays a part in human life. I don’t understand why you feel the need to insist that it’s not the Freudian unconscious. You also quote from Bowlby a letter of condolence that Darwin wrote to somebody about the death of his wife where he says, “I truly sympathize with you, though never in my life having lost one near relation I dare say I cannot imagine how severe grief such as yours must be.”³⁸ You yourself connect this bizarre statement to the facts that Darwin’s mother died when he was eight and that her death was not discussed in the family. At two crucial moments Darwin revealed a block about something of great emotional importance in his experience.

37. John Bowlby (1982), “The Origins of Attachment Theory.” In *A Secure Base: Clinical Applications of Attachment Theory*. London: Routledge, 1988, p. 29.

38. Sulloway, “Darwinian Psychobiography,” p. 30.

FJS: Yes.

PLR: In a footnote to *Born to Rebel*, you say, “I use the term ‘repressed’ advisedly and in a different manner from the the traditional psychoanalytic usage. Bowlby has reformulated the concept of repression as ‘selective exclusion.’”³⁹ I don’t mind using the term selective exclusion if that’s what you want to call it, but it seems to me that these two examples furnish strong evidence for the existence of a dynamic and repressed unconscious. As Bowlby himself acknowledged, his concept of “defensive exclusion” is “no more than repression under another name.”⁴⁰ In Darwin’s blindness during the word game, there’s a process of repression and interpretation going on, from which one can infer the workings of the dynamic unconscious within the self. To me that’s very powerful. Of course, it’s anecdotal evidence, but it provides strong support for a cornerstone of psychoanalysis.

FJS: Clearly, you have much more of a fervor about all this than I do. It sounds like you’re trying to find a way to say, “Well, gee, some of this is still salvageable.”

PLR: Absolutely.

FJS: I don’t feel that need one iota. I do think, however, that there is *some* common ground with psychoanalytic theory in phenomena involving what Bowlby would call selective exclusion. I simply don’t think this kind of evidence proves a dynamic unconscious à la Freud at all, and this is a very important distinction for the viability of the theory as a whole.

PLR: I’m afraid we may be going around in circles. But let’s take one more crack at it. You’ve agreed that Darwin experienced a “mental block” on at least two occasions and that these “blocks” were connected to the death of his mother when he was eight years old. Yet you insist that these examples do not provide evidence for a dynamic unconscious. It’s interesting, by the way, that Bowlby referred not merely to “selective exclusion” but to “*defensive* exclusion” and equated the latter term with repression. Bowlby’s approach is grounded in information-processing theory, but it’s also (as I’ve said) entirely consistent with psychoanalysis. In the essay “The Origins of Attachment Theory” from which I quoted earlier, he wrote, “[T]his

39. Sulloway, *Born to Rebel*, p. 491n62.

40. Bowlby (1981), “Psychoanalysis as a Natural Science.” In *A Secure Base*, p. 71.

revolution in cognitive theory not only gives unconscious mental processes the central place in mental life that analysts have always claimed for them, but presents a picture of the mental apparatus as being well able to shut off information of certain specified types and of doing so selectively without the person being aware of what is happening.”⁴¹

Bowlby then went on to point out that the information warded off by emotionally disturbed or detached individuals is

of a very special type. So far from its being the routine exclusion of irrelevant and potentially distracting information that we engage in all the time and that is readily reversible, what are being excluded are the signals, arising from both inside and outside the person, that would activate their attachment behaviour and that would enable them both to love and to experience being loved. In other words, the mental structures responsible for routine selective exclusion are being employed—one might say exploited—for a special and potentially pathological purpose.⁴²

Thus, in a case such as Darwin’s, which turns on the family’s inability to mourn the death of his mother, we are dealing with a manifestation of “pathological” exclusion, and this is fundamentally different from the subliminal phenomena involved in the tachistoscopic experiments you referred to earlier. In this connection, Bowlby reiterated that “just as Freud regarded repression as the key process in every form of defence, so I see the role of defensive exclusion.”⁴³ Despite the difference in terminology, Bowlby clearly comes down on Freud’s side in the debate over repression. So let me ask you again: Doesn’t Darwin’s failure to recognize the word mother in the word game, as well as his declaration that he never “lost one near relation,” tend to prove the existence of a dynamic unconscious?

FJS: I don’t think it proves a dynamic unconscious in the way that most psychoanalysts construe that concept, and without that version of the concept, psychoanalytic theory doesn’t really work.

PLR: Again, why doesn’t it prove a dynamic unconscious?

FJS: Because I think that people who endorse a *dynamic* unconscious, as opposed to unconscious mental processes more generally, are

41. Bowlby, “The Origins of Attachment Theory.” In *A Secure Base*, p. 34.

42. Bowlby, “The Origins of Attachment Theory.” In *A Secure Base*, pp. 34–35.

43. Bowlby, “The Origins of Attachment Theory.” In *A Secure Base*, p. 35.

positing something that is significantly sealed off by repression from the awareness of the individual—something that causes, as a consequence of this repression, major forms of neurotic illness.

PLR: When Darwin didn't see the word mother, wasn't that sealed off from his awareness at that time?

FJS: Yes, for a few seconds. But the moment someone said to him, as must surely have happened, "Look, Charles, that spells 'mother' and you had a mother, didn't you?" he would have responded [snaps his fingers], "Absolutely." There's not much more you need in order to understand that story, except to say that Darwin appears to have had a mild tendency to dissociate topics about his mother from his most conscious level of thought. Darwin didn't need to undergo five years of psychoanalysis to reach some other deeply hidden level of thought that might finally have freed him from that particular hang-up. Additionally, there is no plausible evidence at all that this dissociation on Darwin's part caused him to have any pathological symptoms. Rather, his mental block and his neurotic symptoms (excessive anxiety) appear to have a common basis in patterns of family dynamics of which he was consciously and fully aware whenever he was reminded of them.

PLR: I think that when someone showed Darwin that the word spelled mother, and he realized with bewilderment that his blindness had somehow to be connected with the death of his mother, he might well have decided—had he lived in the 20th century instead of the 19th—that he needed psychoanalysis. The emotional effect of this early loss and the conspiracy of silence in the family could not possibly have been alleviated by pointing out one manifestation of its consequences.

FJS: True, but none of Darwin's symptoms need be viewed as the outcome of repression in *Freud's* sense of the term or, indeed, in any sense of the term.

PLR: The repression is exhibited by Darwin's failure to see the word when it's staring him in the face.

FJS: I don't like calling it a repression in that sense. You can have a block about something without its being repressed in a pathological manner. For example, Darwin's "repression" can be regarded primarily as a symptom of his disrupted attachment to his mother rather than as a cause of any neurotic problems. That is, the repression itself may not be pathological. And if the repression isn't pathological, then it is not really a demonstration of the validity of Freud's

ideas or of any neo-Freudian ideas that impute a pathological outcome to such mental processes.

PLR: As I've said, I'm not too particular about the term we use. Even if we attribute Darwin's blindness to "disrupted attachment," it's still a function of his "neurotic problems." The actual repression is only a symptom; its underlying cause is the family's inability to acknowledge or mourn the death of his mother. You can see why I think this example is powerful.

FJS: Well, for some events in life and their psychological interpretation there are indeed some ideas that might be called "psychoanalytic" that are perhaps useful. But I don't think they're nearly as powerful a tool in understanding the life course, and the sources of psychopathology, as most psychoanalysts believe. More important, it is imperative to distinguish such ideas as primary and secondary repression from one another and from their supposed consequences in symptom formation, dreams, and memory lapses. In Freud's theories, these ideas were all intimately linked together in an explicitly causal model. In modern psychology, they are not necessarily linked in the same manner. So one can believe in "secondary repressions," which Darwin may have experienced, and not believe that anything like a Freudian account is useful in understanding Darwin's life.

PLR: I'm perfectly willing to settle for "secondary repressions," if that's what you want to call them, since I have no investment in defending Freud's outmoded biology. It's precisely because Bowlby helps us to sort out what continues to be valid in Freud's thought from what has not stood the test of time that I find his work to be so valuable. In your book on birth order you yourself show how the microenvironment of family dynamics shapes a person's outlook for the rest of his or her life. It's a wonderful and thoroughly original argument.

FJS: This argument, of course, wasn't in any way psychoanalytic.

PLR: I know, but it's compatible with psychoanalysis.

FJS: It's about life experience, which may or may not fit with a psychoanalytic account. I tend to think it doesn't, although it certainly accords with a psychoanalytic approach in many important ways.

PLR: Your book brings something to the forefront of consideration that had previously been neglected. It's a great work, and I'm pleased to hear you say we share at least some common ground.

FJS: To the extent that psychoanalysts are interested in human

development there are lots of common areas to discuss about dynamic (that is, conflict-laden) influences in the family. But there is a hard core of old Freudian notions that most analysts still employ that I don't use in explaining how birth order shapes personality. These older psychoanalytic notions are linked strongly to the theory of a dynamic unconscious; whereas when I write about the influence of birth order or parent-offspring conflict on personality development, I am discussing everyday interactions that, over time, become differences in personality. There is no need, in this kind of account, to believe that anything about personality development involves unconscious mental processes that have been repressed.

In my account of birth order and family dynamics I begin with Darwinian organisms that are wired to learn strategies. If a boy grows up with a sister, he tends to experience fewer physically aggressive interchanges with that sibling, and perhaps more affection in the relationship as well, because that's an emotion that is generally more appropriate for brothers and sisters to express toward one another. These kinds of differential experiences, depending on variations in family composition, appear to rub off on personality. A younger brother of an older brother, for example, is more likely than the older brother to be tender minded, fun loving, and unconventional. These kinds of differences arise as a result of differences in sibling strategies, as offspring attempt to adapt themselves to the family milieu. These kinds of nonpsychoanalytic explanations help to elucidate some of the environmental components of human development. Of course, we must also recognize that a substantial portion of the variance in human personality is due to the genetic roll of the dice that primes the pump, so to speak, and accounts for about 40% of this variance.

PLR: You are referring to temperament.

FJS: Yes, I am thinking of the kind of work Jerome Kagan has done on shyness, for example.⁴⁴ Of course, to provide a full explanation one must also factor in all the within-family variance that can be explained by family dynamics, and the various interactions between genetics and environment, and then one must consider influences from outside the family—mentors, good school experiences, and so

44. Jerome Kagan & J. Stephen Reznick (1986), "Shyness and Temperament." In *Shyness: Perspectives on Research and Treatment*, ed. Warren H. Jones, Jonathan M. Cheek & Stephen R. Briggs. New York: Plenum, pp. 81–90; Jerome Kagan, J. Stephen Reznick & Nancy Snidman (1988), "Biological Bases of Childhood Shyness," *Science*, 240:167–171.

on. Then one can, in theory, hope to explain the bulk of the observable variations in personality. From a scientific standpoint, what else does one need? We've done most of the job of providing an explanation without having to appeal to tortured interpretations of dreams in order to discover some set of walled-off repressions from the first three years of life that are driving all subsequent stages of development in ways that are supposedly a mystery to anyone who has not spent years in psychoanalytic therapy. We just don't need this kind of psychoanalytic explanation!

PLR: I agree with so much of what you've contributed that it would be churlish of me to argue with you. [FJS laughs] What I'm trying to do is to get you to agree with some of what I want to say, just as I can agree with what you say.

FJS: I'm a younger sibling, so I don't have a great need to agree. [laughs]

PLR: On the other hand, it might also be a problem if you insisted on being perverse just for the sake of stubbornness. [FJS laughs] You should be open to evidence and willing to agree if there's good reason to do so.

FJS: True, but it's not as if I haven't considered these issues.

PLR: Of course not. But when you speak of "tortured" dream interpretations, does that mean that all dream interpretations are useless? Your description implies that any use of it is an abuse. In *Freud, Biologist of the Mind*, you don't give any attention to the death of Freud's father. You mention it only once in passing in the context of Joseph Campbell's hero myth.⁴⁵ The fact that Freud's father died in October of 1896 as all the other things were going on in Freud's life isn't something to which you attach any explanatory weight. Nor that he lost an infant brother when he was less than two years of age. You don't examine any of Freud's dreams for the light that they might shed on his inner life. Your contribution is to put Freud in the context of the biological thinking of his time. That involves approaching the history of psychoanalysis with the tools and methodology of the history of science. I don't have a quarrel with that. But it leaves out of account those perspectives—whether they be Freudian, contemporary psychoanalytic, or even simply psychobiographical—that try to get at the inner experience of the person.

45. Sulloway, *Freud, Biologist of the Mind*, p. 447.

FJS: I wasn't writing a personal biography of Freud. I was writing an intellectual biography.

PLR: But the personal motivations and the scientific theories are intertwined.

FJS: That's true of Bowlby's biography of Darwin. When you're writing that kind of a biography, you go back and forth between the emotional life and the intellectual life as much as is necessary to tie the two stories together. Bowlby treated more of the emotional life of Darwin than I did of Freud. But I *did* consider aspects of Freud's personality and emotional life whenever I thought they were especially relevant to his development as a psychoanalytic theorist.

PLR: Your presentation of the relationship to Fliess focused on the intellectual content of Fliess's ideas and how they influenced Freud. You argue that it's the "forgotten context" of Fliess's ideas, and "not Freud's intellectual and emotional 'bondage,'" that explains his acceptance of Fliess's scientific work.⁴⁶ In other words, you take issue with the psychoanalytic model of looking at the Freud–Fliess relationship as a transference relationship.

FJS: Yes, I certainly do.

PLR: That would involve setting it in the context of a lifelong emotional pattern, at least on Freud's side. Let's say we were to take Charcot, Breuer, Adler, Jung, and the other major male figures in his life and trace his reactions to them. I think doing that would have the effect of highlighting the irrational elements in his thinking and the less attractive features of his personality. Your presentation of the Freud–Fliess relationship focuses so much on the intellectual plane that it leaves out the emotional dynamic, whether we want to call it transference or something else. The death of Freud's father isn't factored at all into what's going on in his relationship with Fliess. Just as your interest in Freud wasn't connected to anything in your own life—you didn't come to psychoanalysis in any kind of introspective mode, but saw it simply as a problem in intellectual history—so your treatment of Freud leaves out the subjective or phenomenological sense of his inner experience.

Richard Webster's book, which forms the successor to yours as a major critique of Freud, highlights Freud's compulsive need for fame and the Messianic quality to his thinking.⁴⁷ Freud's megalomania

46. Sulloway, *Freud, Biologist of the Mind*, p. 147.

47. Richard Webster (1995), *Why Freud Was Wrong: Sin, Science, and Psychoanalysis*. New York: Basic Books.

caused him to disregard evidence, not to engage in peer review, to be contemptuous of those who dared to disagree with him, and so forth. All that is very deplorable in Freud, but these failings can perhaps be attributed to Freud's insecure relationships to his parents. At least we can see that the formation of his theory is connected to his personal psychic needs. Webster's argument could therefore paradoxically be said to vindicate the essential tenets of psychoanalysis, even though he's pointing out Freud's flaws as a human being.

FJS: Just to reiterate, *Freud, Biologist of the Mind* was an intellectual biography, and I used and appealed to the psychology of Freud's own life whenever I thought it was relevant to the broader story of understanding how this particular man developed this particular set of theories at this particular time. Like Webster, I placed enormous weight on the psychological theme of Freud's grandiosity and need for fame. I built the third part of my book, which deals with the myth of the hero, around this whole topic. I felt that the various character traits in Freud that were responsible for his Messianic sense of ambition and his intolerance of criticism had exerted a major influence on the content and style of his science and on the nature and style of his friendships.

PLR: So we agree. Then why not say that Freud's relationship to Fliess is to a great extent transference?

FJS: I didn't think that was necessary or useful to be said.

PLR: But you took issue with that interpretation.

FJS: I took issue with the attempt by psychoanalysts-turned-historians to sweep Fliess completely under the carpet as an embarrassment to the story of the hero by using the notion of transference.

PLR: But it doesn't have to be an either/or, does it?

FJS: No, but I should make it clear that I do not believe that the notion of "transference" adds anything to what is already obvious about Freud's relationship with Fliess. Theirs was a very strong friendship and scientific collaboration long before Freud's father died. One simply doesn't need to appeal to the death of Freud's father to understand why that friendship was steaming along and why the two were so busy exchanging scientific ideas. Besides the obvious fact that the two men liked each other enormously and had corresponded for nearly a decade before Freud's father died in 1896, there was also a shared world view and a shared scientific logic to their friendship. It is also true that a big part of that friendship rested on an emotional component, as does any long-term relationship. There

they are, two grandiose guys who are feeding each other's ego. [PLR laughs] Equally or perhaps even more important, however, is that their friendship and collaboration nurtured an exciting way of thinking that involved Darwin's legacy to the 19th century—a legacy that both of them understood, but that Kurt Eissler, Max Schur, and Ernest Jones didn't. It was this Darwinian and biogenetic logic that made Freud think that Fliess wasn't a crackpot—that he was in fact a genius—and it's also the same logic that made many of Fliess's contemporaries agree that he might well be a genius. Many other contemporaries did consider Fliess to be a crackpot, but Freud wasn't in this other group. It is important to keep in mind the big picture in assessing this friendship. Fliess was an ear, nose, and throat specialist with a passionate interest in biology, and Freud was himself an accomplished biologist. So the two of them could bounce biological and other scientific ideas off one another. They had a deep and shared interest in various books on late 19th-century biology and medicine that are rarely mentioned in Freud biographies but that lined Freud's own bookshelves and that he read, annotated, and cited. This was the common intellectual bond that underlay their friendship.

Then this shared scientific outlook began to focus more and more on Freud's own work. Freud and Fliess began to collaborate. As long as one can tell the full story about this intellectual resonance in their friendship, one doesn't need to say much more about it to understand the birth of psychoanalysis and to trace key aspects of this new system of ideas back to Fliess's own theories and publications.

Believe me, if I had thought I could get useful mileage out of some dream Freud experienced, or a specific interpretation that he reached about one of his dreams, or the fact that his father died in 1896, I would have made the most of such information, as I occasionally did for other aspects of Freud's life and as I have sometimes done in my work on Darwin's life and ideas. But I didn't see a way to engage in such discussion without degenerating into the kind of convenient speculation, so common in psychoanalytic biography, where commentators weave a tale that says whatever they want it to say, using endless "reconstructions" of biographical evidence. Peter Swales has occasionally played this game in some of his articles, building on material offered by Freud himself (as in the "*aliquis*" episode that Freud reports in *The Psychopathology of Everyday Life*).⁴⁸ I don't

48. Peter J. Swales (1982), "Freud, Minna Bernays, and the Conquest of Rome: New Light on the Origins of Psychoanalysis." *New American Review*, 1(2/3):1-23.

think one really needs or benefits from this kind of quasipsychoanalytic speculation. It could be right. It could also be totally wrong. Who knows? I believe that these kinds of arguments are just taking up space on paper without really getting us anywhere.

The real issue, then, is, to what extent should one appeal to psychobiography in doing biography? Forget about whether it's psychoanalytic or not. To what extent do biographers need to appeal to complicated explanations about the interior life of their subjects? One has to consider this issue on a case-by-case basis. I don't ever rule it out, but I tend to draw on it very selectively. For me, psychobiographical claims have to seem reasonable and eminently plausible. They also ought to be testable—if not for the individual case being considered, at least for other cases more generally.

PLR: I'm all in favor of hypothesis testing in science, but, when it comes to offering an interpretation in therapy or of a work of art, I don't think one can resolve questions of meaning in the same way that one might be able to test a hypothesis about the relative influence of social class and birth order on orientations toward an intellectual revolution. As Bowlby argued, psychoanalysis is both a natural and a historical science. This distinction originated with Wilhelm Dilthey and is fundamental to hermeneutic theory. Psychoanalysis is a natural science in its attempt to formulate general principles of personality development and psychopathology, but it is a historical science in its attempt to understand and treat the problems of specific individuals. And, as Bowlby has written, whereas in the natural sciences "we are dealing with statistical probabilities," when it comes to history or literature "the individual example is the very essence of the case."⁴⁹ I thus don't see an incompatibility between the type of hypothesis testing that you advocate and exemplify in researching certain types of problems and the necessarily more subjective types of interpretation that also need to be undertaken with attention to evidence, tact, and an appeal to something shared in the public arena.

My concern is that in your espousal of a natural-science model, with its reliance on statistical methods, you may be losing sight of the equally important perspective brought to bear by the humanities. The writing of history or literary criticism, for example, isn't simply free association when it is done well. In fairness to Peter Swales, moreover, I would point out that his interpretation of the

49. Bowlby, "Psychoanalysis as a Natural Science." In *A Secure Base*, pp. 75-76.

“*aliquis*” parapraxis as a piece of disguised autobiography, indicating that Freud may have had an affair with his sister-in-law, Minna Bernays, is supported by copious archival research and external evidence. I think we can fault the excesses of psychobiography—whether psychoanalytic or otherwise—without concluding that the idea of trying to explain the interplay between intellectual development and the inner life is futile. I hope we are agreed on this. But in addition to the intellectual bond between Freud and Fliess, to which you rightly draw attention, there was undoubtedly a strong emotional component. According to Webster, the theory of hysteria is completely unsound, and, if we look back to Freud’s adoration of Charcot, we can see that he brought to the relationship with Fliess a predisposition to validate his own sense of Messianic identity by projecting it onto someone else. I think there is a lot of good evidence for that.

FJS: I think there’s just as much evidence to say that they had simply a strong friendship—nothing more, nothing less. I don’t see Freud as projecting anything onto Fliess. By all accounts, Fliess was a fascinating person. For this reason, Freud didn’t need to project anything onto him. He would need to project onto Fliess only if Fliess were an idiot, because then Freud’s biographers would have to explain why this brilliant man, Freud, was associated with someone who had all these embarrassingly wrong ideas.

PLR: From my point of view, transferences play a part in all close relationships, in which an irrational component is inevitably present. That Fliess was intrinsically fascinating doesn’t preclude the possibility that Freud projected his own fantasies onto him. The same could be said of Charcot or Jung. Both were distinguished men, but that alone doesn’t explain why Freud should have responded to them as intensely—and, indeed, irrationally—as he did. And what about the Eckstein episode, in which Fliess failed to remove a meter-long piece of gauze from his patient’s nasal cavity after an operation? Surely that didn’t cast Fliess in a very good light. Didn’t Freud need to do some pretty serious rescuing of his idealization of Fliess in the aftermath of that?

FJS: Well, in what way? Yes, it was an embarrassing episode and Freud certainly made apologies for Fliess. Almost anybody would do that for a good friend.

PLR: Or one might begin to think, “This guy wasn’t all that I cracked him up to be. I need to be more critical.”

FJS: One does well to keep in mind that Freud himself made mistakes in medical practice that were just as bad as Fliess’s. Freud, for

example, admitted to one mistake in medical judgment that actually killed one of his patients.⁵⁰ In any event, the friendship went on for a considerable time after this episode. One really ought to try to separate the fact that, as a physician, Fliess slipped up from the fact that, as an intellectual, he had ideas that were very interesting and compelling to Freud and many others. For these and other reasons, I don’t see the Emma Eckstein episode as being all that important to their relationship. True, Fliess made a major mistake in leaving a big wad of gauze inside his patient’s nose. That was clearly a sign of some incompetence. Still, because we don’t know that much about Fliess’s medical career, we also don’t know if this was an isolated episode. I think Freud probably treated it as though it was a one-time, rather embarrassing mistake. Indeed, Freud blamed the patient’s hemorrhage after the gauze was removed on her pathological need for sympathy, or some such thing. Now, other commentators can choose to make Fliess’s blunder into a big deal, and virtually every psychoanalyst who writes on Freud has done so. But the same people have been so busy trying to denigrate Fliess in the service of his “transference” role in Freud’s life that they have completely missed, in my opinion, the forest for the trees. The forest involves their scientific collaboration.

PLR: A lot of people who are critical of Freud also make a big deal of the Emma Eckstein episode. So it cuts both ways. The fact that Freud blamed Emma for causing her own hemorrhage, instead of holding Fliess accountable for his blunder, is precisely my point. And if Freud’s defense of Fliess was motivated to some degree by guilt over his own medical blunders, that supports a transference interpretation. As Max Schur long ago argued about Freud’s dream of Irma’s injection, which he saw as connected to the Eckstein episode, “the main wish behind Freud’s Irma dream was not to exculpate *himself* but Fliess. It was a wish not to jeopardize his positive relationship with Fliess.”⁵¹ Freud’s identification with Fliess gets to the emotional or irrational components of their relationship.

FJS: What’s irrational about it?

PLR: That Freud was unable to see how far-fetched Fliess’s ideas were about the connection between the nose and the female sexual

50. See Freud (1900), *The Interpretation of Dreams. Standard Edition*, 4:111–112. London: Hogarth Press, 1953. As Peter Swales points out in this volume, the name of the patient in question was Mathilde Schleicher.

51. Schur, *Freud: Living and Dying*, p. 12.

organs. And that he blamed Emma for her postoperative bleeding.

FJS: You sound like Kurt Eissler! We can also express this question in a very different manner. Why did Fliess hang around with somebody whose ideas were often as far-fetched and wrong-headed as Freud's were? Does the answer to this question require that we assume that Fliess's father had recently died and that he was undergoing some sort of transference during his efforts to come to terms with his father's death, which then prompted him to project his greatness onto Freud? Of course not.

PLR: As a matter of fact, Fliess's father died in 1878, when Fliess was nearly 20. We don't know what kinds of psychological effects this loss may have had. But it would certainly be plausible to think that Fliess had a lot of emotional needs invested in Freud too. We're looking at it from Freud's point of view. The dynamics of transference don't come into play only after the death of Freud's father. They can be seen as early as Freud's first letter to Fliess, dated November 24, 1887, when he wrote in the opening paragraph that "you have left a deep impression on me which could easily lead me to tell you outright in what category of men I place you."⁵² Even when he barely knew him, Freud cast Fliess as a hero, the same "category" in which he aspired to place himself. This is transference. Were we to try to tell Fliess's side of the story, we would, of course, have to learn a great deal more about his life. But just because the circumstances are different, that doesn't make him immune from transference.

FJS: My whole point is this: Fliess's ideas seem far-fetched today, but I don't view them as far-fetched in their historical and biogenetic context. These seemingly wacky ideas were no more far-fetched than Freud's ideas were, and some people actually thought that they were far more plausible than Freud's theories. Freud eventually became more famous, but that doesn't necessarily make him more right. The notion that a 28-day period is fundamental to human physiology has lots of support from the obvious evidence of the menstrual cycle in humans to Darwin's arguments that the gestation stages for animals tend to be in multiples of a week. The place that Fliess went badly astray was in his conviction that he was objectively documenting these periodic phenomena in clinical and other life-course data, but this conviction is no more weird or irrational than the extremes to which Freud sometimes went to interpret his patients' symptoms and dreams. We have all read patients' accounts of the dogged way

52. In Masson, ed., *Freud-Fliess Letters*, p. 15.

that Freud would wait for one little clue and then say, as he did to Clarence Oberndorf, "Well, your dream about going in a carriage with a white and black horse means you want to marry a black woman."⁵³ That's just gratuitous and even a bit arrogant. That style, however, is no different from Fliess's style. Freud was a conquistador, but so was Fliess. Some scientists display this grandiose style; others don't. Moreover, there are times when scientists who manifest this style come up with brilliant ideas that are actually true.

In 1896 or 1897, neither Freud nor Fliess could know whether the other was right or wrong because neither was using rigorous methods of hypothesis testing. There was no adequate check on their ideas. One doesn't need to go into the question of whether a transference was going on. Theirs was simply a friendship between two people who were both very smart and very ambitious, who shared this "conquistador" style, and who could not find too many other people to listen to their far-reaching ideas. By bonding together, they were able to further each other's career by a constant cross-fertilization of ideas. In addition, although neither of them was terribly critical of his *own* ideas, they tended to help each other in important ways by providing a critical sounding-board for the other's speculations. This, in any event, is the gist of the collaborative relationship that I portrayed in my book. Unfortunately, almost every attempt to "do the transference thing" in Freud biography is motivated by the same faulty logic you have expressed: Why did Freud, a brilliant man, spend so much time with this idiot named Fliess?

PLR: I don't think I said that exactly.

FJS: You referred to the "irrational components" of their relationship. It's on the tape! And then you said that "Freud was unable to see how far-fetched Fliess's ideas were." Some psychoanalysts have even called Fliess "paranoid" on account of his "pathological" theories.⁵⁴ That's the wrong way to think about their relationship.

PLR: But I have stressed that these "irrational components" were involved on both sides. As far as I'm concerned, whatever may be said

53. See Abram Kardiner (1977), *My Analysis with Freud: Reminiscences*. New York: Norton, p. 76. The dream was presented to Freud during the first analytic hour by Oberndorf, a Southerner raised by a black nanny. Oberndorf refused to accept Freud's interpretation that his reluctance to marry stemmed from an uncertainty whether to choose a white or a black woman. The analysis foundered, and Freud came to view Oberndorf with disdain.

54. Sulloway, *Freud, Biologist of the Mind*, p. 141.

of Fliess, Freud certainly had a paranoid streak. After all, the relationship came to an end over Freud's accusation, in his letter to Fliess of August 7, 1901, that "you take sides against me and tell me that 'the reader of thoughts merely reads his thoughts into other people,' which renders all my efforts valueless."⁵⁵ A decade later, the Schreber case,⁵⁶ where Freud attempted to explain paranoia as a result of repressed homosexual impulses, is still haunted by the shadow of Fliess. But in your account, it's as though Freud and Fliess had a purely scientific collaboration in which there was no irrational element. And though I'm quite willing to agree with you that not all Fliess's ideas were ludicrous, while some of Freud's were indeed outlandish, it remains true that Fliess is a nearly forgotten figure today whereas Freud was arguably the most influential thinker of the 20th century. Thus, I think that the question of why Freud valued Fliess so highly continues to be worth asking. For me there's no inherent conflict between an intellectual friendship and emotional needs.

FJS: But that's a completely different issue from needing a "transference" to explain the irrational element in their friendship. This concept assumes that there was something irrational about that friendship and collaboration. Yes, there was something emotional about it, but that truth is unfortunately seized on by analysts to take another unwarranted step, namely, to say that the friendship was irrational; and this fundamentally unjustified next step is then used in an effort to sweep Fliess under the carpet.

PLR: Where I would come out is not to sweep Fliess under the carpet, but to point to a lifelong pattern in Freud's intellectual style that in many ways reflects poorly on Freud. The relationship to Fliess was one of the most important in Freud's life, but the way that he behaved toward Fliess is consistent with what we know about his behavior toward many other males in his life.

FJS: What part of the relationship reflects poorly on Freud?

PLR: If we take the relationship to Jung as a comparison, Jung was clearly a person of immense gifts.

FJS: I don't really agree. I am reminded of the old adage that "Jung makes Freud look scientific." In any event, let's go back to Fliess. What in *that* relationship reflects badly on Freud?

55. In Masson, ed., *Freud-Fliess Letters*, p. 447.

56. Sigmund Freud (1911), *Psycho-Analytical Notes on an Autobiographical Account of a Case of Paranoia (Dementia Paranoides)*. In *Standard Edition*, 12:9–82. London: Hogarth Press, 1958.

PLR: I agree with you about Jung, but scientific rigor is not the sole criterion for greatness. As far as Freud and Fliess are concerned, I suppose that what casts Freud in the most negative light, besides the Emma Eckstein business, is the controversy over plagiarism that erupted in the aftermath of the relationship. As even Ernest Jones conceded, Freud was dishonest in not admitting to Fliess that he had divulged his ideas about bisexuality to Swoboda, who then passed them on to Weininger.⁵⁷

FJS: I'm not sure myself that this particular episode reflects terribly badly on Freud. Moreover, by that time (1902–1904), the relationship was already estranged. Hence this episode does not shed much light on the earlier period, when their collaboration was so remarkably intense. Also, there are some other interesting historical aspects to the issue of bisexuality that makes this episode, in which Freud supposedly facilitated the plagiarism, appear to be blown out of proportion. First of all, the idea was not unique to Fliess, though he certainly liked to think it was. The notion of bisexuality had been written about by lots of people before him. Fliess didn't discover this idea; he didn't own it. Freud was therefore free to talk to anybody about it. There's nothing unusual about his communicating these ideas to a patient.

What is unusual is that, when Fliess wrote to Freud to try to find out how Weininger had learned about bisexuality, Freud did not immediately admit to Fliess that he had been the conduit for this information. But I can well imagine that, in such a situation in which people have become estranged and one (Fliess) suspects the other (Freud) of doing something improper, Freud's first inclination was to try to smooth over the situation by making light of it and by avoiding all the embarrassing details. And I think that is just what happened. Freud wrote his letter to this effect, and then he received another letter from Fliess that called his bluff.

Freud's disingenuousness was not exactly in the category of a white lie—it's more serious than that. But it wasn't a felony either. All in all, I don't see Freud's momentary duplicity as reflecting particularly badly on him. Normally, when one hears a statement like "the Fliess relationship reflects badly on Freud," this judgment is being applied to the earlier period, where psychoanalysts have long felt the need to explain why Freud tolerated this guy. Naturally, for psychoanalysts, the answer has got to be "the transference" and Freud's father's death, and then we are back into the standard psychoanalytic whitewashing technique.

57. Jones, *Life and Work of Freud*, 1:315.

PLR: I think one could explore these issues without whitewashing Fliess or anyone else. I mentioned Jung. We know that one of Freud's fainting spells occurred in the Park Hotel in Munich—a city he associated with Fliess, and possibly in the same hotel room where they had their final quarrel. It had to do with Freud's claim that Jung had not cited his work. Freud told Jones that there was "a piece of unruly homosexual feeling at the root of the matter."⁵⁸ As far as Adler and Stekel are concerned, he wrote to Ferenczi, "Adler is a little Fliess *redivivus*, just as paranoid. Stekel, his appendage, is at least called Wilhelm."⁵⁹ These examples suggest that Freud had a compulsive tendency to cast his relationships in repetitive patterns.

FJS: This is an interesting issue. Freud was obsessive about priority, as are most original people. Fliess was that way too. So we have two highly ambitious personalities duking it out for scientific priority as highly territorial and self-confident people typically do. It is not surprising that, when the relationship finally foundered, it did so over the issue of scientific priority. Freud had begun using some of Fliess's ideas, especially in his thinking about psychosexual development and repression, and he had a substantial lapse in 1898 when he failed to recall that Fliess had told him about one particularly important idea on the subject.

PLR: It had to do with bisexuality.⁶⁰

FJS: Again, this particular idea—at least in its most basic form—wasn't unique to Fliess, but Fliess was the vector by which Freud first became exposed to it. Normally, in a scientific collaboration one would cite the other person if that other person had supplied some particularly important point of information that would otherwise have remained unknown to the first person. It would be like my saying, "I thank my friend Stephen Jay Gould for enlightening me on the issue of rapid evolutionary change," when I might have gone to several other people and gotten much the same information. But if Steve Gould had first told me about this important point I would probably have felt obligated to cite him, especially if he, like Fliess,

58. Jones, *Life and Work of Freud*, 1:317.

59. Letter of December 16, 1910. In Eva Brabant, Ernst Falzeder & Patrizia Giampieri-Deutsch, eds. (1993), *The Correspondence of Sigmund Freud and Sándor Ferenczi, Volume 1, 1908–1914*, trans. Peter T. Hoffer. Cambridge, MA: Harvard University Press, p. 243.

60. Freud (1901), *The Psychopathology of Everyday Life. Standard Edition*, 6:143–144. London: Hogarth Press, 1960.

had put some original twist on it. Freud didn't do that. Clearly, he was ambivalent about the fact that in their joint scientific thinking they were collaborating more and more. As I expressed this issue in *Freud, Biologist of the Mind*, there came a point where the question began to be: Whose theories explained whose? The two men's ideas had become so intertwined. For example, Freud's whole theory of psychosexual development was initially inspired and informed by Fliess's notion of periodic sexual thrusts. Infantile sexuality was very much a Fliessian construct. They began having conversations in which Fliess would say, "Maybe it isn't your interpretations that are responsible for the patient's recovery. Maybe it was because you conducted your analysis on a periodic day." The two psychosexual theories were competing to explain much of the same information about pathological fixation points, the onset of repression, the specific manifestations of neurotic symptoms, and the reasons for recovery and relapse. All the key disputes in that relationship, and the eventual estrangement, were really about priority, and territorial boundaries generally. So it's not surprising that many of Freud's subsequent disputes with colleagues involved the same basic issues. The bugaboo in Freud's character and failed friendships was not really Fliess. It was Freud's intense need for originality.

PLR: Quite right. But the relationship with Fliess is caught up in that dynamic of Freud's. One can see it in the "*non vixit*" dream, where Freud dreams of outliving Fliess and he recalls a childhood fight with his nephew John in which both boys claimed to "have got there before the other."⁶¹ This is not to minimize either Fliess's intellectual contribution or the importance of that relationship to Freud on its own terms, but it does suggest that it fits into a broader pattern in Freud's life.

FJS: In that event, it's not that Fliess reflects "badly" on Freud, but instead that, in engaging in a relationship with Fliess, Freud sometimes exhibited behaviors that reflected badly on himself.

PLR: Exactly. That's what I'm trying to say. But if there's any silver lining to this cloud, it's how those of Freud's actions which reflect badly on him can be explained in terms of psychoanalytic theory. We can see that his conflicts over priority go back to his childhood.

FJS: Well, we can "see" that only because Freud, who held a theory to this effect, gave us the selective biographical information that confirmed his theory. Moreover, almost any theory of human development

61. Freud, *The Interpretation of Dreams. Standard Edition*, 5:483–484.

can explain the same general thesis. We don't need Freudian theory to understand that Freud's desire to be first, and to be intellectually dominant, must have had *something* to do with his early life. We could go over to Harvard University, call the entire Department of Psychology together, supply them with these same biographical facts and ask, "Can you account for the behavioral continuity in this pattern?" They're virtually all going to say "yes," and, to my knowledge, none of them except Philip Holzman has ever been psychoanalyzed. So you're going to be outvoted 30 to 1 on the question of whether psychoanalytic theory—either orthodox or revisionist—is in any way required to explain this aspect of Freud's adult behavior! The real question is, can psychoanalytic theory make a point that all those smart Harvard professors cannot make about the same biographical evidence? Show me that the theory can, and I'll perk up with considerable interest.

PLR: I guess I'd like to talk to Philip Holzman. [laughs] But this meeting of the Harvard Psychology Department falls into the category of hypothetical examples. The notion that Freud deliberately filtered the evidence about his life in order to lend credence to his theories seems far-fetched to me. After all, Freud tried to persuade Marie Bonaparte to *destroy* the Fliess correspondence, which provides the record of his self-analysis.⁶² You seem to be of two minds about whether Freud's struggles for priority went back to his childhood. On one hand, you criticize the psychoanalytic approach as incapable of explaining anything; but, on the other, you say that its genetic model is a commonplace shared by other psychological theories. I would say that psychoanalysis is participating in a common discourse, and, if what can be argued from a psychoanalytic point of view overlaps with what people might argue from somewhat different explanatory frameworks, so much the better. But just as Darwin's symptoms of repression in adulthood are tied to the death of his mother, it's important to me to maintain that the patterns in Freud's life can be traced back to his early experiences and unconscious conflicts. The "guilt of the survivor" motif is one that he himself linked to childhood experience. A plausible case can be made that what was driving Freud's behavior was rooted not simply in his character but also in his early experience. Although on one level he was able to articulate this awareness consciously, the behavior that he exhibited repeated the pattern despite his intellectual insights. He told Fliess that he rejoiced in the "*non vixit*" dream at having outlived him and about his need to find the loved friend and hated enemy in the same person.

62. Masson, Introduction to *Freud-Fliess Letters*, pp. 4–11.

I'm not saying that psychoanalysis is the only vocabulary available to talk about these things, but it seems to me to be an illuminating way of framing the data.

FJS: It doesn't do much for me. I've read so many of these tortured interpretations and reinterpretations of Freud's dreams that it all seems a bit like bad food warmed over too many times. I don't know of a reliable way to evaluate the truth of such claims. Maybe there's some genius out there who can do this sort of psychospeculation delicately and with a reliable flair for getting at the truth, but most people who do it simply end up with stories that so blatantly confirm psychoanalytic theory that it's almost sickening. Of course, there's a ready audience for those stories among the true believers, but this circumstance is neither here nor there.

PLR: As I've said, I want to be rigorously critical about psychoanalysis. But the criteria for evaluating truth claims in humanistic fields such as history and literary criticism, to which psychoanalysis as an interpretative discipline is closely allied, cannot be the same as in the natural sciences. To be sure, much of what Freud maintained is no longer credible and has to be tossed into the dustbin of history. So for me it becomes imperative to try to sort out what has withstood the test of time from what hasn't. In your essay "Reassessing Freud's Case Histories," you say: "Psychoanalytic theory is perhaps unique in the history of science in that it contains within itself an elaborate historical account of its own intellectual origins. Freud has become his own most famous 'case history.'"⁶³ I agree with that. Of course, it's impossible to test the interpretation of a dream as though it were a scientific hypothesis.

FJS: One cannot test *any* of these hypotheses when doing biography. One can't even test these hypotheses in the clinical setting, as Adolf Grünbaum and others have persuasively argued.⁶⁴ That's why the most important methodological step in psychobiography is to draw from those healthy domains of science which study human development. If we were to survey academics and ask them what we need to know, almost none of them would tell us that it is psychoanalysis.

63. Frank J. Sulloway (1992), "Reassessing Freud's Case Histories: The Social Construction of Psychoanalysis." In *Freud and the History of Psychoanalysis*, ed. Toby Gelfand & John Kerr. Hillsdale, NJ: The Analytic Press, p. 159.

64. Adolf Grünbaum (1984), *The Foundations of Psychoanalysis: A Philosophical Critique*. Berkeley: University of California Press.

PLR: I think it depends on the discipline. I would agree that any psychoanalytic proposition that has been scientifically refuted is not likely to be useful in writing psychobiography or anything else. Too many people in the humanities go on using such concepts as the death instinct, primary narcissism, the pleasure principle, and so forth, which in my view are clearly outmoded. But once we have laid a firm scientific foundation, there are still questions of meaning that science alone cannot answer. This is where the humanities come in. The problem with Grünbaum is that he thinks everything can be reduced to a natural-science model. For him, there is no difference between the “history” of a particle in physics and the history of a human being. Many people writing biography today still find psychoanalysis has a contribution to make.

FJS: Yes, and humanists more generally are indeed drawn to it. But, pardon me, what the hell do they know? They’re not scientists. They’re not in a position to test and validate such theories. That’s why the last refuge of psychoanalysis is in the humanities, where its practitioners are less equipped to tell the difference between a valid and an invalid scientific theory. If we are ultimately going to do biography right, we need a reliable theory of human development. The specialists who work on this enormously complex problem are developmental psychologists, behavioral geneticists, and the like. None of these people use psychoanalytic theory in any formal sense. They may occasionally fall back on notions that would also be employed by psychoanalysts. That doesn’t necessarily make these psychologists’ accounts psychoanalytic, but it does mean that there’s occasionally a common ground—one that lies mostly in common sense.

PLR: Exactly. And that’s something. I can’t say I share your attitude toward the humanities. But I’m all in favor of learning from developmental psychology and other branches of sciences. Indeed, Daniel Stern, a psychoanalyst renowned for his work in developmental psychology, explicitly states that when these two perspectives come into conflict, “it is psychoanalysis that will have to give way.”⁶⁵ And there’s no doubt that some of Freud’s interpretations are tortured. It seems to me that when he interpreted the dreams of others, he was often high-handed and imposed his preconceptions on what he heard. As Richard Webster points out, he conjectured that the dream of one of his female patients about a butcher could be explained by his own

65. Daniel N. Stern (1985), *The Interpersonal World of the Infant: A View from Psychoanalysis and Developmental Psychology*. New York: Basic Books, p. 17.

associations to the phrase “Your meat-shop is open,” referring to a man’s trouser-fly, and asserted that an unnamed vegetable must be asparagus, with its phallic connotations.⁶⁶

FJS: The same objection is true of Freud’s interpretations of his own dreams.

PLR: There I disagree. Earlier I used the example of the “*non vixit*” dream, where Freud traced his relationship to Fliess back to his early childhood, in which there was a literal competition over priority with his nephew John, toward whom he felt an ambivalent mixture of love and hate. This interpretation of his relationship to Fliess and his survivor guilt makes much more sense to me than does his interpretation of his patient’s dream. Freud’s explanations are generally far more compelling when they arise out of his own experience than when he’s talking about somebody else.

FJS: I would agree, but this still doesn’t mean that any of the interpretations that he offers are correct. They are simply less blatantly arbitrary.

PLR: But how can we determine that?

FJS: We can’t—that’s the whole point. All Freud’s free associations to his dreams were influenced by his theories. Moreover, we know only about those free associations he chose to tell us about. As a result, we cannot make a determination about the validity of Freud’s theories through any of the anecdotal evidence that is available about Freud’s life or, for that matter, about anybody else’s life. We must do scientific research to get a handle on these kinds of questions, which include memory confabulation, correlations in behavior over time that are not strictly “causal,” and various other enigmas of human development. Unfortunately, we will find almost none of this research being conducted by psychoanalysts, so they’re not going to help us where it really matters.

PLR: I admire your commitment to the scientific method. But I think it would be a delusion to suppose that controversies over the interpretation of historical events can ever be settled empirically. The same goes for psychoanalysis. To take a concrete instance, how are we going to prove that there was something significant in Darwin’s failure to recognize that when you put “m” before “other” you get the word “mother”? Or when he said he had never experienced the loss of a near relation? The facts of what happened to Darwin are not in

66. Webster, *Why Freud Was Wrong*, pp. 267–269.

dispute, but the meaning is up for grabs. Surely we have to rely on our judgment here.

FJS: Right, but there's no amount of theory now known in psychology—and there will probably never be such theory in the future—that will allow us to take a biographical example such as these two and say that we've proven anything. They are purely anecdotal examples, and science doesn't prove anything by means of anecdotes. So all we can really say is that an anecdote is consistent with some other body of research where we *are* able to prove things. In psychology, almost all these kinds of proofs are conducted at the population level rather than the level of the individual. For this reason, the proofs are generally statistical. This is what Bowlby did when he argued that individuals who lose a parent at a certain age tend to develop a cluster of problems, including being more prone to anxiety and panic attacks.⁶⁷ The proof of the pudding in such hypotheses now lies in large epidemiological studies with adequate controls. From these kinds of controlled studies, scientists can say statistically that some of those hypotheses are valid and some of these hypotheses are invalid. But one can still never prove that such hypotheses are true in any individual case, no matter how intuitively plausible the evidence may seem.

PLR: We don't disagree about that. Where we seem to differ is in your confidence that questions of value and meaning can be settled with statistics. As you've just said, statistical studies are done at the population level. But in the human sciences, as Bowlby pointed out in a passage I quoted earlier, "the individual example is the very essence of the case." What is more, Bowlby went on to state that history, "whether it deals with societies or persons or ideas, is always concerned with appallingly complex sequences of highly specific interacting events which no amount of science can enable us to explain adequately, let alone predict."⁶⁸ Thus, Bowlby by no means relied exclusively on quantitative methods. From my point of view, the problem with your approach is not that it's scientific—I'm all in favor of science—but that you want science to answer questions about individual cases that call out instead for hermeneutics.

FJS: I have a discussion of this methodological issue in *Born to Rebel*. Anecdotes, as I stress, don't prove anything, though one may usefully employ them to illustrate points that one has already proven

67. John Bowlby (1980), *Loss: Sadness and Depression*. London: Hogarth Press.

68. Bowlby, "Psychoanalysis as a Natural Science." In *A Secure Base*, p. 75.

by other means. Anecdotes, moreover, can be an important source of hypotheses. I am far from wishing to reduce human behavior to statistics, nor did I mean to imply that Bowlby went that route either.

PLR: Fine. So there is a congruence between what we can glean from the human and the natural sciences. You asked me earlier what humanists know . . .

FJS: I meant "knowing" about the intricacies of human development, as understood by science. Humanists know lots of things, and they often have remarkable sensitivity and perception on certain topics. But I was commenting about knowledge in science, not about "knowing" in general, which often involves forms of knowledge that are untestable and hence unscientific, or at best prescientific.

PLR: Surely there's also something to be learned about psychology from literature. Let's say we're confronted with a Shakespeare play and are trying to determine what a passage means. Or what the play as a whole might mean. These are issues of interpretation, and I don't see how questions of meaning can ever be sorted out using the model of hypothesis testing.

FJS: No, they can't. And as far as I know, nobody ever said that they could be.

PLR: But it still seems to me that the endeavor to understand and interpret works of literature is a valuable enterprise. Trained people can judge whether it is done more or less successfully, even though they might not agree about a given interpretation. It's like being a juror in a court case—or even assessing the outcome of a statistical study—where one tries to weigh all the available evidence and to come to a conclusion, but one can never exclude the possibility of being in error in any given instance. The techniques of interpretation in the humanities, without being incompatible with the sciences, have a contribution to make in the effort to understand the meaning of experience in literature and perhaps even in life.

FJS: The similarity between the fields, as I see it, is as follows. Both science and nonscience have to start somewhere, and the first step is to try to reach a reasonable hypothesis about the subject of study. So, if one is interpreting Shakespeare, one studies his plays in great detail. Then one tries to put everything together and come up with an interpretation, and that's fine as far as it goes. The problem is that, in the humanities, one normally cannot test the validity of such hypothetical interpretations, whereas in science the ball game has only just begun when one has finally reached a plausible interpretation. It's at this crucial point that scientists engage in a second step

of research, namely, hypothesis testing. This formal step is missing from most humanistic research because there is no way to carry it out using humanistic or hermeneutic methods. It is this second step that allows us to determine whether a plausible (or implausible) “interpretation” of the data has any validity. Humanistic claims are therefore scientific claims without the science to back them up. Such claims are conjectures, nothing more. Humanists often think they are testing their ideas, but such informal methods are almost always exercises in “confirmatory” hypothesis testing—that is, looking for supporting evidence.

PLR: I don’t think that claims in the humanities can be equated with scientific claims. They may rest on a scientific foundation and should not be contradicted by any known evidence, but they’re of a fundamentally different order. And a good reader, like a good researcher in any field, is open to evidence that disconfirms his or her initial expectations. In my view, psychoanalysis is a hybrid discipline. In its ideal form, if there be such a thing, it has to be accountable to the criteria of science . . .

FJS: Quite right. But it unfortunately isn’t.

PLR: Well, but it could be.

FJS: In theory, yes; but given the personnel who work in this intellectual domain, the prospects are dim. The problem lies with the training. One can’t become a psychoanalyst without partaking of the institutional mechanisms the discipline has established, and that, to put it bluntly, is usually the end of someone’s career as a scientist.

PLR: Well, it could be detrimental, but I’d like to think it need not be fatal. [laughs] Let me quote you something from Edward O. Wilson.

FJS: He’s a kind man, so perhaps he can help you.

PLR: That’s a virtue. [FJS laughs] He says: “Psychoanalytic theory appears to be exceptionally compatible with sociobiological theory. . . . If the essence of the Freudian revolution was that it gave structure to the unconscious, the logical role of sociobiology is to reconstruct the evolutionary history of that structure. When Freud speculated in *Totem and Taboo* on the primal father, primal horde, and the origins of the incest taboo, he created a sociobiological hypothesis, but a poor one.”⁶⁹ I’d agree with that.

69. Edward O. Wilson (1977), “Biology and the Social Sciences.” *Daedalus*, 106:135–136.

FJS: So would I, as I explicitly argued in *Freud, Biologist of the Mind*.⁷⁰

PLR: In principle, then, psychoanalysis is compatible with science. Maybe you’ve decided that psychoanalysis is not productive or interesting for you, but to me it’s critical to ask what still holds up.

FJS: What precisely is there that still holds up? I’d love to know.

PLR: I’m arguing that the idea of a dynamic unconscious still seems to be valuable.

FJS: I’ll mostly reject that claim for the various reasons I have given. What’s the next one on your list?

PLR: I tried to defend that idea by pointing to the sequence of incidents from Darwin’s life brought forward by Bowlby.

FJS: I don’t view those incidents as evidence for the dynamic unconscious, at least in the theoretically and medically strong form that Freud espoused and that was central and necessary for his whole theory of psychosexual development and psychopathology. These kinds of biographical stories are far more plausibly seen as evidence for a far milder version of the unconscious, one that Freud would not himself have considered “psychoanalytic.”

PLR: We may not agree about the terminology, but in some form it surely exhibits repression.

FJS: Once again, I don’t like the word, just as Bowlby didn’t like it either, because it implies certain aspects of Freud’s theoretical system that have not stood the tests of subsequent scientific inquiry. In any event, we are certainly not dealing here with repression as part of the dynamic unconscious, as Freud himself understood it. It’s such a watered-down version of the original idea. Freud would be outraged.

PLR: I don’t think so. And as I’ve tried to demonstrate, I think you’re wrong in claiming that Bowlby rejected the notion of repression—though he preferred to call it “defensive exclusion”—just as he clearly agreed with Freud about the dynamic unconscious. Unfortunately, we can’t resuscitate either of them to ascertain whose interpretation of their respective positions is closer to the mark. And I’m still interested in considering the role of early experience. I think Freud himself is a good example of the utility of genetic explanation, since we know so much about his life and his early childhood—sibling loss . . .

70. Sulloway, *Freud, Biologist of the Mind*, pp. 5, 367, 500.

FJS: Right there you are committing a gigantic methodological error. You have a pet idea, and then you go to one life—Freud’s—to confirm it, just as Freud himself did.

PLR: I’m testing a hypothesis. And you yourself just said that “biographical stories” were “evidence” of *something*, even if we disagreed about exactly what that was.

FJS: No, you can’t test a hypothesis with evidence drawn from the life of a single person.

PLR: We could falsify it, I suppose.

FJS: No, you can’t even falsify it. Although you may be able to find a case that doesn’t fit your hypothesis, that doesn’t disprove your hypothesis. You can’t do anything scientific with a single life. Even evidence from multiple biographies does not, *per se*, provide a *scientific* perspective on such claims. One needs to perform statistical tests on such evidence, using large samples carefully controlled for the kinds of covariates (and alternative hypotheses) that are relevant to the claim under consideration. Let me provide a concrete example. Let us suppose that the early death of a sibling, as in Freud’s case, is thought to lead to “survivor guilt,” which, in turn, is thought to cause ambivalence in adult relationships, which, in turn, is thought to nurture an excessive credulity toward wacky ideas, as well as a desire to steal those ideas. We could test this idea by examining a large population of subjects according to their beliefs in a variety of pseudoscientific ideas and popular superstitions. We might then look for a significant correlation between such beliefs and the early death of a sibling, as well as a significant correlation between both of these variables and an objective measure of ambivalence in adult relationships, perhaps by using an instrument drawn from the research on attachment theory. Finally we might try to replicate and extend these results (assuming they turned out to be favorable to our initial hypothesis) by studying priority disputes in science. There we would expect people who lost siblings in childhood to be overrepresented. In addition, we would expect the same people to exhibit a lack of critical thinking when faced with new and highly speculative ideas (such as Fliess’s theories) that have subsequently turned out to be incorrect.

Because behavior is “overdetermined,” as Freud liked to point out, none of these relationships, if they exist at all, will be perfect. Indeed, we know empirically that such relationships, even when valid, tend to explain only a modest percentage of the evidence, which is why we need to employ large samples and formal statistical tests in our

empirical analyses. Finding a case in which little ambivalence existed in adult relationships, and in which desire for priority did not go hand-in-hand with uncritical thinking about a friend’s ideas, is no more a disproof of the theory than confirming evidence is a proof of these same claims. Such anecdotal evidence cannot decide the validity of our theory, and a hundred anecdotes are no more useful than is one in this regard. All that such anecdotes can do is to suggest plausible hypotheses, some of which may be testable (and thereby become amenable to scientific analysis) and some of which may not be.

The problem with the general anecdotal approach that you are advocating and trying to employ yourself is that, with scores of relevant examples, one invariably ends up overweighting “confirmations” and underweighting “inconsistent evidence” concerning the hypothesis that originally motivated the research. Many experimental studies have shown this to be the case, and the remarkable progress of modern science, since scientific methods were introduced into research in the 17th century, dramatically demonstrates the superiority of formal methods of testing over those generally employed in the humanities. Humanists may be content to engage in suggestive “interpretations” in their own academic research, but when they fall desperately ill it is modern medicine, not 16th-century medical hermeneutics, that saves their lives.

Not only do psychoanalysts and people within the humanities more generally not understand that anecdotes are not the basis of science (or truth), but they often have an additional incorrect belief that people can conduct formal testing within the psychoanalytic situation. This is an issue that Grünbaum has written about very effectively.⁷¹

PLR: What you’ve described sounds to me less like a scientific hypothesis than a polemical narrative. I agree that science is progressive in a way that art is not, but that doesn’t mean we can dispense with everything that isn’t science. I’m open to weighing the evidence critically.

FJS: The scientific method is hardly a “polemical narrative.” Rather it is a set of formal procedures for *minimizing* polemics by allowing them to be resolved as efficiently and rapidly as possible. In your own humanistic researches, I am sure that you are open to weighing evidence in a critical manner, but weighing the evidence when the *N* equals 1 doesn’t count for much in science. You’re not doing science, even if you *are* engaged in reasonably rational thinking.

71. Grünbaum, *The Foundations of Psychoanalysis*.

PLR: Which is not necessarily incompatible with science. If you can't perform a scientific experiment when $N = 1$, the corollary is that quantitative methods can't possibly tell us how to interpret any individual case, which is the most important thing in history or any of the other *human* sciences. This goes back to what I was saying before about psychoanalysis as a hybrid discipline.

FJS: But what ends up happening with this kind of one-step proto-science is nicely exemplified by the many attempts written by true believers to salvage psychoanalysis. Christopher Badcock in England, for instance, has tried to show in this general manner that psychoanalysis is compatible, after all, with current Darwinian biology.⁷² What we often get from such attempts, however, is merely a translation of current ideas about evolution back into standard psychoanalytic terms, which is little more than an attempt to save the phenomena, so to speak. That's what happens when people think they are doing hypothesis testing and allying themselves solidly with modern science, but are not really doing science at all. Under these circumstances, people just rationalize the evidence in ways that suit their fancy, and the only really relevant evidence to them is *confirming* evidence.

PLR: I don't know Badcock's book, but his project sounds very worthwhile to me. It's not a matter of "saving the phenomena" or translating everything back into "standard psychoanalytic terms," but of seeing what in psychoanalysis can be integrated with modern science. You seem to imagine that your own thinking is unclouded by subjectivity, while proponents of psychoanalysis are incapable of evaluating a hypothesis in the light of the evidence. But though this may have been true of Freud, who embraced science in theory while often betraying it in practice, I've agreed that much of what he said needs to be rejected.

FJS: That's not going to help you much, however. The problem is that, since you are not doing science yourself, you're at the whim of whatever the last decade or two of psychoanalytic "research" has said on the subject. Unfortunately, in psychoanalysis people go from one fad to another. Morris Eagle and David Wolitzky have written a very good and relevant paper about theory change in psychoanalysis.⁷³

72. Christopher Badcock (1994), *PsychoDarwinism: The New Synthesis of Darwin and Freud*. London: HarperCollins.

73. Morris Eagle & David Wolitzky (1989), "The Idea of Progress in Psychoanalysis." *Psychoanalysis and Contemporary Thought*, 12:27-72.

Yes, the theories do change over time, but almost never for the reasons that cause theories to change in the hard sciences.

PLR: But, as I've been arguing all along, psychoanalysis is not entirely a hard science. It is equally grounded in hermeneutics. If we're seeking to understand an individual case, scientific methods alone are insufficient.

FJS: Psychoanalysis is not a hard science at all. It's not! It's not even a soft science!

PLR: Insofar as Bowlby's work reflects a tradition that grows out of psychoanalysis, I see it as compatible with science.

FJS: Yes, there are topics that some psychoanalysts work on that scientists also work on. But the fact that the problem areas overlap doesn't make psychoanalysis a science or its practitioners scientific.

PLR: If something can be refuted by scientific investigation, I do not want to continue to employ its ideas or premises.

FJS: I'm sure that's true. But the problem you and other humanists face is that, if you're not an active participant in science, you must passively sit and wait for the real scientists to come up with their results.

PLR: Well, we're all indebted to the work of others. We're all experts only in limited areas. When we consider what we think we know, we can't help relying on authorities to a great extent. If I am persuaded by your work on birth order, for example, it's not because I have done all the computations and statistical analyses myself. But I have read your work and I am convinced that the evidence you offer is good evidence and that it's been presented in a logical and a valid way. Surely that's acceptable to you, isn't it?

FJS: Yes, at one level. I appreciate the fact that you respect my work on this subject and have, to a considerable extent, been convinced by it. But the fate of my ideas on the subject of birth order and family dynamics will ultimately be decided by the scientific community. If this community goes against me, the support of nonscientists such as you won't help me one bit.

PLR: I would say that the fate of your ideas, like that of Freud's or anyone else's, will be determined by their reception by the culture as a whole, a process in which scientists are important, but by no means the only, arbiters. Admittedly, I'm not doing either the clinical or the experimental work on the epistemology of psychoanalysis

myself, but I am trying to be conversant with it. I feel justified in concluding that certain things can be dismissed because they've been refuted, and certain other things seem to hold up pretty well.

FJS: I made these comments about the nature of science only because you indicated that you believed you could do some kind of testing in an *informal* way. For the reasons I have given, I don't believe this is possible. In addition, I don't believe that the feedback you may get from psychoanalysts is worth much, because it is inherently unscientific. Unfortunately, most scientists are working on other problems, so you won't get much feedback from them either.

PLR: I'm interested in feedback not only from psychoanalysts but also from people who question psychoanalytic assumptions. That's why I'm talking to you. I was asking whether we might be able to use Freud's life as evidence for psychoanalytic theory.

FJS: I think this effort, however fascinating, is just *doomed* to failure, because the evidence, being anecdotal, can only suggest hypotheses, not confirm or disconfirm them.

PLR: I respect the scientific model of hypothesis testing for which you are arguing, but there are questions of value and meaning that science alone can't answer. As I've already stipulated, any proposition that can be refuted by scientific investigation must be rejected.

FJS: Well, building theories based on single biographies has got to be number one on the list of things to reject. It's just a bad strategy—inherently unscientific. You can go about constructing a biography out of anecdotes, but this procedure will never in any really effective way change your psychoanalytic thinking. You're more likely to confirm your thinking than to refute it.

PLR: How might we set up an experiment to examine whether or not the experiences of the first three years of a person's life have an influence on his or her subsequent development? It's hard for me to imagine how that could be tested. Wouldn't we have to rely on natural experiments?

FJS: The natural experiments one must use have to be epidemiological studies. This kind of research involves studying large numbers of individuals who have gone down different developmental pathways. Such research requires the simultaneous control of all relevant covariates. Then one asks, is there continuity in behavior as measured during the first three years of life and then when measured later on? It's difficult to do these kinds of studies because one

must take into account genetic inputs and other factors. Probably the most rigorous way to tackle this question is through a behavioral genetics design in which developmental psychologists measure and study behavior longitudinally. Such studies have already been done. As a result of such research, the evidence is already available, and it strongly indicates that the first three years of life do not have the kind of predominant influence on behavior that Freud himself believed.

PLR: And Darwin.

FJS: Darwin's comment on this whole subject was a little more open ended. For him, the influence of the first three years was not as great as Freud believed. And one should bear in mind that this comment was something he only said once. How exactly did Darwin put it?

PLR: I have the passage here. He said: "It is a virgin brain adapted to receive impressions, and although unable to formulate or memorize these, they nonetheless remain and can affect the whole future of the child recipient."⁷⁴

FJS: That claim is not terribly far reaching. He was just saying that the brain absorbs a lot of information and experience in the first three years. He was not saying that the first three years are the absolute key to life as an adult. Freudian theory makes a much stronger statement on this subject.

PLR: Well, Darwin did say that these experiences, even though they may be below the level of consciousness, "can affect the whole future of the child recipient." That sounds pretty Freudian to me. But I'm simply asking, what can we learn from the study of Freud's life? We know so much about it.

FJS: Once again, I think that's really the wrong place to start if you want to understand human development more generally. Individual lives can suggest interesting hypotheses, and such lives can illustrate theories of human development that have been validated by other means; but individual lives cannot be employed to test hypotheses, such as Freud's claims about the importance of the first three years of life.

PLR: We may have here a difference in our disciplinary training. Again, you seem to be saying that the only function of history or literature is to illustrate truths already established in the sciences, but

74. Quoted in Sulloway, "Darwinian Psychobiography," p. 29.

they are not sources of knowledge in their own right. I'm interested in working in a way that exhibits consilience and a convergence of knowledge, but the humanities have a distinctive contribution to make. And the humanities work—as therapy does—with individual cases. We need good scientific theories to bring to bear on our work with each unique case. But, when we try to interpret a dream or the experience of a relationship, it's very hard to reduce that to something statistically demonstrable. We're dealing with something inherently elusive and subjective. Perhaps I could use this as a transition to ask you a question about Paul Robinson's chapter on your work.

FJS: I haven't actually read it, although I know it's largely negative. [laughs]

PLR: Robinson says, "According to Sulloway, Freud will be great if he can be made to resemble Darwin. It will not do to suggest that his achievement was more like that of Augustine or Rousseau."⁷⁵

FJS: I think that's silly.

PLR: Could you explain why? I think Robinson raises an important issue. In highlighting Freud as a cryptobiologist, you situate his work in relation to one important domain of cultural discourse, but you don't look at Freud in relation to a tradition of introspection or consider his literary and philosophical background.

FJS: That's true, but your quotation from Robinson implies that I think Freud has to be salvaged by allying him with Darwin. That wasn't the goal of my book. In many ways, I saw Freud and psychoanalysis as beyond the point of being salvageable. In any event, the book's goal was simply to understand Freud, not to make him somehow more palatable by making him into a Darwinian (which he was not).

PLR: There's nothing in Robinson to suggest that it's a matter of trying to "salvage" Freud. His point is that what you value in Freud is his biological thinking and his participation in the Darwinian tradition, and nothing else.

FJS: I think that's a somewhat misleading reading of my book. My goal was not at all to make Freud great by making him look like Darwin or by highlighting his debt to biology. My goal was to understand why Freud developed his various ideas, and how the most

important of these ideas grew out of a 19th-century evolutionary tradition. The net result of this analysis was to highlight the problematic character of Freud's theories, not to make Freud "great." Robinson has missed the whole point.

PLR: I think Robinson was responding to those passages, which we've already discussed, where you say that Freud offered "the most comprehensive evolutionary explanation of the origins of human behavior" or your quotation from Philip Rieff that he produced "the most important body of thought committed to paper in the twentieth century."⁷⁶ Surely your understanding of Freud, which included paying tribute to his greatness where you felt it was appropriate, led you to connect him above all to Darwin.

FJS: Let me try to clarify this point, because it is an important one. There's no question that I valued those aspects of Freud's thinking that were rooted in natural science much more than I did those which were rooted—to take the extreme opposite—in wild speculation and mysticism. There is, as you know, that side to Freud. There is also a side to Freud that is nonscientific in a more virtuous sense, and that is his vast knowledge of literature, archeology, and the humanities more generally. I didn't write a lot about the contributions of literature, linguistics, philosophy, and so on to psychoanalytic theory because I have a particular viewpoint on this whole subject, one that I believe is both useful and defensible. The core of Freud's psychoanalytic theories—the key postulates that really make them work at a causal, natural-science level—is drawn largely from the medical biology and psychiatry of his day. These key postulates involve such notions as fixation, regression, "organic repression," ontogenetic developmental processes recapitulating phylogenetic developmental processes, and, finally, hydraulic forces that move dynamically within the nervous system. This core set of "metapsychological" ideas is very consistent, well articulated, and well thought out. In addition, these key ideas are linked to certain underlying assumptions of his day that Freud and others considered quite plausible. For example, given the enormous influence that Helmholtz's theory about the conservation of energy exerted in Freud's day, Freud's model of the nervous system made this particular idea central to all of his psychoanalytic theorizing.

Now, laid over this core paradigm in the corpus of the *Standard Edition* of Freud there is a widespread appeal to literary illustrations and allusions and other examples of high culture. The question

75. Paul Robinson (1993), *Freud and His Critics*. Berkeley: University of California Press, p. 67.

76. Sulloway, *Freud, Biologist of the Mind*, pp. 367, 358.

is, did Freud's extensive knowledge of literature, philosophy, and the culture of the day filter back into the core of his psychosexual theory and influence it in a fundamental way?

PLR: That's an excellent question. How would you answer it?

FJS: In *Freud, Biologist of the Mind*, I deliberately and specifically identified all the things that I thought illuminated and explicated the core of the theory. Given the enormity of this task, I did not spend much time discussing aspects of Freud's intellectual style that he used only to illustrate the theory or to embellish it in a culturally erudite fashion. In answer to your question, I was never able to find evidence that Freud's knowledge of linguistics, for example, exerted a fundamental influence on some of the deep assumptions of his theories the way that John Forrester, among others, has argued.⁷⁷ I don't find Forrester's account very plausible. It ignores a fundamental fact about Freud's career, namely, that he spent the first half of it as a neurobiologist and neuroanatomist, not as a psychoanalyst and certainly not as a linguist.

For me, the bottom line about Freud's intellectual development is that he trained from the age of about 18 through his mid-40s in neurobiology and to some extent evolutionary biology. Those were the fields in which he intended to make his career, with all the natural scientific values and identifications that went with these fields of study. Freud read a vast amount in these disciplines, and it was out of these biological and medical fields that he knew he would have to draw all the basic assumptions and working principles for a theory of human development that was really adequate to the task. In other words, Freud firmly believed that the theory of human development had to be a *scientific* theory and, for this reason, it had to draw its deepest roots from psychobiology and medicine. So he fashioned his key assumptions out of those biomedical domains, and *that's* the story I tried to chronicle.

By contrast, Freud did not go the route of thinking to himself, "Well, I'll contemplate Goethe's writings for a few days and draw some fundamental assumptions about human nature and human mental functioning from them," because he knew that one couldn't formulate a scientific paradigm out of Goethe's ideas. He could and did use Goethe, who was one of his favorite authors, to his advantage in other ways, just as he used other German writers for the same

literary purposes, but none of these authors became crucial sources of the key assumptions and principles of psychoanalytic theory.

PLR: Goethe, of course, like Leonardo, was a scientist as well as a humanist and served Freud as a model in this respect. And let's not forget that Freud wrote *On Aphasia*⁷⁸ and used linguistic as well as biological models throughout his career. You don't have to be a Lacanian to know that psychoanalysis is called the *talking* cure! Because you see everything through the lens of Freud's desire to construct a scientific theory grounded in biology, you don't present his self-analysis as a psychological process of self-discovery in its own right. For you, literature serves merely to illustrate or embellish an already established scientific truth. But don't the literary and philosophical dimensions of psychoanalysis have to be considered along with Freud's scientific ambitions? When Freud wrote to Fliess on October 15, 1897 that "a single idea of general value" had occurred to him, he immediately cited Sophocles' *Oedipus* and Shakespeare's *Hamlet*, and he invoked these two works again in *The Interpretation of Dreams*.⁷⁹

FJS: Those are literary examples, and damn good ones, which Freud used to illustrate an insight that he had derived by thinking about (a) the things his patients were telling him and (b) the theory of psychosexual development.

PLR: And also what he was finding in his own self-analysis.

FJS: Yes, although I think a lot of that personal exercise consisted of reading into his own life experiences an organizing and explanatory structure that he was then in the course of deriving from other sources.

PLR: But there's a process of investigation occurring on several tracks—his clinical work, his reading, and his analysis of his own dreams and early life history. I think it's significant that, when he pulled this all together, he did so by mentioning Oedipus.

FJS: How could any educated person, once he had finally reached that particular insight, not mention Oedipus? I don't view a knowledge of Sophocles' *Oedipus Rex*, which Freud had read as a gymnasium student, as in any way *causing* the development of

78. Sigmund Freud (1891), *On Aphasia: A Critical Study*, trans. E. Stengel. Madison, CT: International Universities Press, 1953.

79. Masson, ed., *Freud-Fliess Letters*, p. 272; and see Freud, *The Interpretation of Dreams. Standard Edition*, 4:262.

77. John Forrester (1980), *Language and the Origins of Psychoanalysis*. New York: Columbia University Press.

psychoanalytic theory. The relationship with Fliess and the associated collaborative insights into a dynamic and biogenetic conception of psychosexual development—these are primary causal determinants of Freud's thinking. In particular, his familiarity with Haeckel's biogenetic law caused Freud to develop a sweeping paradigm about human psychosexual development—to recognize, for example, that the child will be “polymorphously perverse.” Freud could have read a million novels and Greek tragedies and never reached that crucial conclusion about childhood sexuality. But such ideas arose almost inevitably from a familiarity with 19th-century medical science, particularly sexology, where they were already being discussed by such people as Albert Moll and Wilhelm Fliess.

PLR: The kind of discussion we are having here exemplifies why I think that quantitative methods are insufficient in historical research. We have to use our judgment in weighing various possible influences on Freud's thought. Because you are steeped in the history of science rather than in literature, you believe that Albert Moll was more important to Freud than Sophocles, Shakespeare, or Goethe. Didn't Freud's reading of *Hamlet* and *Oedipus* cause him to think along certain lines?

FJS: Please don't misunderstand me. I have not said, nor do I believe, that quantitative methods are sufficient for understanding history. What I have said is that anecdotal evidence—the main thrust of historical argumentation—is insufficient when it comes to formal hypothesis testing, which is the main thrust of scientific argumentation. After a careful study of Freud's readings in the emerging discipline of sexology, as it was then being developed by such figures as Albert Moll and Richard von Krafft-Ebing, I reached my conclusion that Moll's work was more influential on Freud's ideas about psychosexual development than was the work of Shakespeare or Goethe. Freud needed the general ideas in this emerging field to make theoretical sense out of otherwise puzzling or unconnected empirical findings, including those garnered from his study of literature. Without this theoretical perspective, together with the medical evidence supporting it, Freud's reading of *Hamlet* is exceedingly unlikely to have caused him to formulate a theory that the whole of human development, and its various psychopathological glitches, is *Hamlet* writ large.

PLR: Or that incest is a fundamental component of human desire?

FJS: No, I don't believe that kind of causal imputation about *Hamlet* is the correct historical scenario. If it were, and if, on average, one

psychiatrist in a thousand was likely to think about psychopathology in similar terms, Freud's own ideas would have been anticipated by tens of thousands of medical practitioners during the three centuries that preceded his own development of these ideas. *Hamlet* and *Oedipus Rex* are simply not enough. But having grown up in the context of the late-19th-century Darwinian viewpoint on human development, someone like Freud could say [snaps his fingers] that this kind of perspective supplies one with a scientific theory about human development. *Then* the examples from literature take on a new and very different meaning. The fundamental causal chain proceeds, then, from biology and medicine to clinical psychology (including Freud's self-analysis) and then to the relevance of admittedly powerful literary illustrations.

I think this whole question of cause and effect in Freud's thinking comes down to the following difference in opinion between us. People such as I, who have a natural sciences background, would like to ground their story of Freud's intellectual development in the natural sciences. People who are trained in the humanities naturally want the humanities to play a more central role in the story. Now, as I'm sure you will agree, it doesn't matter what any of us may want; all that matters is what actually happened. And Freud himself was solidly grounded in, more than anything else, the natural sciences, the domain of knowledge that gave his theories, as he once commented, their “indispensable organic foundation without which a medical man can only feel ill at ease in the life of the psyche.”⁸⁰ That was Freud's core identity. He was trying to develop what he personally considered to be a natural science. He was extremely concerned that his theory be a reductionist scientific theory. He would have hated the notion that psychoanalysis is a fundamentally hermeneutic discipline, although hermeneutics played an important role in his thinking and methods.

PLR: You've warned against the danger of giving too much weight to confirmatory evidence. Yet you don't seem to realize that there is anything problematic in equating your version of the story of Freud's intellectual development with the truth. “What actually happened” to Freud, to use your words, as in history generally, is by no means transparent but is inevitably subject to interpretation. In addition to the passage you quoted, Freud also wrote in the 1935 Postscript

80. Letter of April 19, 1908. In *The Freud/Jung Letters: The Correspondence between Sigmund Freud and C. G. Jung* (1974), ed. William McGuire, trans. Ralph Manheim & R. F. C. Hull. Princeton, NJ: Princeton University Press, pp. 140–141.

to *An Autobiographical Study* that, “after a lifelong *détour* through the natural sciences,” his interests had “returned to the cultural problems which had fascinated me long before, when I was a youth.”⁸¹ So I think your conception of Freud’s “core identity” as simply that of a natural scientist is too narrow, though this was of course one component of his extraordinarily complex make-up. As early as *Studies on Hysteria*, Freud said that it struck him as strange that his case histories “should read like short stories and . . . lack the serious stamp of science.”⁸² Hence, not only was Freud more than a natural scientist, but his texts have to be understood with the tools of literary criticism.

FJS: That particular remark in *Studies on Hysteria* is neither here nor there. Such a statement is equivalent to Freud’s saying that anecdotal evidence, in and of itself, isn’t science even though it’s often great literary material that helps to illustrate the general truths of science. He felt that what he was doing was strange precisely because he was trained in natural science and strongly identified with this training.

PLR: That’s true, but he was employing a discourse that came out of literature.

FJS: Not exactly. Like any clinician, Freud found it necessary to illustrate general medical principles by using stories, and that was a far cry from having been trained to explore the neuroanatomy of *Petromyzon*. But arriving at the kind of certainty of scientific knowledge exemplified by the neuroanatomy of *Petromyzon* is really the level at which he ultimately wanted to do science, even if he had to reach that level of theoretical certainty by illustrating his theories with case histories that seem like episodes from a novel.

PLR: Again, you’re reducing literature to the status of a decorative illustration rather than treating it as a source of knowledge in its own right. Freud was a *writer* as well as a scientist, and he often found himself taking the side of the poets against the scientific wisdom of his day.

FJS: But that’s not causal. Rather, it’s largely *tactical*, in my view, at least. With any behavioral theory one can think of, there has almost

81. Sigmund Freud (1925), *An Autobiographical Study*. *Standard Edition*, 20:72. London: Hogarth Press, 1959.

82. Josef Breuer & Sigmund Freud (1895), *Studies on Hysteria*. *Standard Edition*, 2:160. London: Hogarth Press, 1955.

always been some poet or other who’s said much the same thing. So if one happens to be in the scientific minority by having advocated a set of unpopular theoretical viewpoints, one can usefully cite a famous poet who said much the same thing and then say, “Well, you see that everything I have found was said before me by great poets.” That sort of effective tactic is neither here nor there when it comes to the origin of Freud’s biomedical theories.

The fact is that Freud, as I have said before, was trying to base his theories solidly in natural science. At the same time, he was also a smart, highly literate fellow who naturally did his best to dress up his theories by illustrating them with as many literary embellishments as he could. If one is attempting to write an intellectual biography of Freud, one needs to go beyond Freud’s literary allusions to account for the fundamental axioms that empower his theory of psychosocial development. These fundamental ideas include his theories about fixation, repression, regression, psychosexual stages, the whole of the libido theory, and so on. In addition to all this, one needs to account for the origins of those closely related ideas that make up Freud’s theory of dreaming, where one encounters his appeal to mental hydraulics, dynamic mechanisms such as condensation and displacement, and similar concepts that Freud borrowed mostly from contemporary neurphysiology. These ways of thinking are closely allied to Freud’s *Project for a Scientific Psychology*.⁸³

How this complicated theoretical system of interlocking ideas works to explain the neuroses—*this* is the very core of psychoanalytic theory. This set of biomedical ideas is also the heart of what, as a biographer, one needs to account for. What I tried to do in my book was to focus directly on this conceptual core and pay careful attention to the influences that allowed this intellectual core to emerge. There is no way that this conceptual core to Freud’s psychoanalytic theorizing can be explained by his readings of poets, playwrights, novelists, or philosophers. The key ideas at this core came from 19th-century medical biology. Believe me, if I had found some Greek tragedy that Freud had read and that I felt had exerted a fundamental, causal influence on his development of his theory, I would have said so.

PLR: How about *Oedipus Rex*?

83. Sigmund Freud (1895), *Project for a Scientific Psychology*. In Marie Bonaparte, Anna Freud & Ernst Kris, eds. (1950), *The Origins of Psychoanalysis: Letters to Wilhelm Fliess, Drafts and Notes: 1887–1902*, trans. Eric Mosbacher & James Strachey. New York: Basic Books, 1971, pp. 355–445.

FJS: [pause] I think that, if Sophocles had never lived, Freud would still have developed the Oedipus complex as a theoretical notion. Now, what he would have called this complex is another matter entirely. Obviously, he would not have been able to avail himself of Sophocles' name for the mythical Theban king. But Freud would surely have found another name for this concept.

PLR: I doubt that the Oedipus complex by any other name would have smelled as sweet. [laughs]

FJS: Still, Freud did not need Sophocles to come up with the basic concept. Other people in Freud's day had discussed this whole issue, including Westermarck, who dealt with the question of incest avoidance from an anthropological perspective.⁸⁴ Freud could have read all about this general topic without ever having had any knowledge of that wonderful Greek tragedy by Sophocles. So I don't view Freud's knowledge of this play as being at all central to his most fundamental psychoanalytic claims. Of course, the fact that the play is so well known and provided a universal illustration of the concept is clearly important at another level, but it does not appear to have caused Freud to have hit on the idea in the first place. It certainly did, however, allow him to communicate this idea in a particularly effective manner. At least that's my view on this matter.

PLR: But Westermarck's theory that the incest taboo stems from an innate aversion among people reared together is the opposite of Freud's, for whom it is a prohibition on unconscious desire. And when Freud wanted to assert the universality of his vision of early childhood experience, the Oedipus myth as presented by Sophocles was indispensable to him.

FJS: I agree, although I would say that the Oedipus myth was "particularly useful" to Freud rather than being "indispensable."

PLR: Still, the Oedipus complex exemplifies Freud's fusion of scientific and literary cultures. This fusion becomes part of psychoanalysis.

FJS: Again, I agree, as long as we are clear that this fusion occurred largely at the level of *communicating* the theory rather than causing the theory to arise in the first place.

PLR: Freud's style of drawing universal generalizations on the basis

84. Edward A. Westermarck (1891), *The History of Human Marriage*. 3 vols. London: Macmillan, 1921.

of particular cases is itself indebted to a strategy that we could call philosophical or literary. He finds something to be true of himself, he finds it in a patient, and he sees it to be enacted in a literary work, and therefore he can say that it is the fate of all of us to have this desire.

FJS: I don't know if I really buy that interpretation. First of all, a style of reasoning that generalizes from a single case is also generally called bad science. Freud had his considerable shortcomings as a scientist. These shortcomings included drawing generalizations consistent with a preconceived theory, which he then failed to test adequately, rather than simply generalizing from too few observations. It was the preconceived theories that tended to make single observations so powerful in Freud's mind. The Oedipus complex is a case in point. To conceive the idea of the Oedipus complex one doesn't need much more than to think that the child is sexual but must repress most of that premature sexuality. As Freud first conceived of them, the child's forms of sexuality are perverse and in conflict with civilization. This, incidentally, was one of Fliess's bioenergetic and biogenetic theories, as Freud himself acknowledged in print.⁸⁵ The energy for higher social and mental functions comes from the repression of sex during childhood, which repression, in turn, produces "sublimation" of this sexual energy. Therefore, if the child is truly sexual, the child will inevitably have sexual feelings toward the opposite-sex parent, and this kind of sexual attraction clearly has to be repressed. Well, now you're talking about the Oedipus complex.

PLR: Does the biogenetic law explicitly involve incest?

FJS: No, not directly. Freud was the one person who came to the conclusion that one could merge the two concepts together and end up with the basic logic of *Totem and Taboo*.⁸⁶

PLR: The two concepts being?

FJS: The first one is that ontogeny recapitulates phylogeny—not just *any* phylogeny, but the sexual phylogeny of our species. This means that the child is destined to pass through adult sexual stages of all our ancestors.

PLR: And the second concept?

FJS: Then Freud needed the notion of the repression of incestuous

85. Sulloway, *Freud, Biologist of the Mind*, pp. 175–179.

86. Sigmund Freud (1913), *Totem and Taboo. Standard Edition*, 13:1–161. London: Hogarth Press, 1955.

feelings. Of course, Freud reached that particular idea in part through his clinical work, although his preconceived theories also played a role in the kind of clinical “evidence” he encountered. He became convinced that repression is a source of all neuroses. Then he became convinced that the only targets of repression are sexual feelings. This meant that the only true cause of neuroses in Freudian theory had to be the repression of sexual feelings or experiences. Now enter the biogenetic law. Given this evolutionary principle, what becomes repressed is inevitably the “polymorphously perverse” nature of sexuality in childhood. What, then, are some of the specific forms that this perversity takes? Oral, anal, and also incestual. Finally, oedipal conflict could be related directly back to Darwin’s ideas, in *The Descent of Man*, about mankind’s original living conditions in a “primal horde” ruled by a single dominant male.⁸⁷

PLR: And so the emphasis on incest—right or wrong—is Freud’s contribution?

FJS: Yes, to a certain extent, although, like so many of Freud’s ideas, this one has its precursors (in the work of Westermarck, for example). For Freud, the importance of incestuous impulses arose almost the moment he abandoned the seduction theory and accepted Fliess’s alternative viewpoint, namely, that the infant possesses its own internal, periodic sexual chemistry. Sooner or later, Freud was bound to realize that one of the great potential conflicts inherent in such a self-generating kind of infantile sexuality was that sexual impulses may be expressed toward a parent. According to Freud, these sorts of inappropriate sexual impulses were going to have to be repressed, and that, of course, is the essence of the Oedipus complex. In short, as soon as Freud started down that biogenetic route, this particular aspect of his theories was in the cards.

PLR: If it was all so inevitable, one might wonder why no one else had arrived at this synthesis already. Did Fliess talk much about fantasy? He certainly had the concept of infantile sexuality, but I wonder whether Freud’s emphasis on fantasy isn’t distinctive.

FJS: I don’t think there’s any evidence from Fliess’s writings, one way or the other, about what he thought on the subject. I guess he would have tended to be less interested than Freud in fantasy and to have interpreted sexual fantasies as the sublimated derivatives of the bio-rhythmic processes that he believed to be driving the developmental system.

87. Sulloway, *Freud, Biologist of the Mind*, p. 372.

PLR: So Freud’s idea that “there are no indications of reality in the unconscious”⁸⁸ is again something that, right or wrong, would have been distinctively his?

FJS: Yes, there’s no evidence that Fliess had any thoughts on that particular subject. Still, Fliess’s general theory of human development strongly implied that psychic reality (fantasies) could be just as clinically relevant to psychopathology as life experiences. As soon as Freud began to elaborate a psychosexual theory of development that included an active Fliessian core to it and is “recapitulatory” in the Haeckelian sense . . .

PLR: Not the Hegelian sense? [laughs]

FJS: No, Haeckelian.

PLR: I know.

FJS: Well, I am glad we don’t have to haggle over Hegel. [laughs] Within this biogenetic perspective, the issue of the extent to which a child’s sexual experiences are largely psychological rather than just physical was inevitably going to arise. It’s really not much of a conceptual leap. The crucial step for Freud was when he decided that fantasy could sometimes have an importance even greater than reality. Why did he take that step? I think he did so for a very simple reason: there weren’t enough cases of childhood sexual abuse or “seduction,” as he often termed those experiences, to create the number of “neurotics” found in society because (a) seduction couldn’t be *that* widespread and, more important, (b) many people who were known to have had sexual experiences as children did not become ill as adults. Those two conclusions, which Freud spelled out in his September 21, 1897 letter to Fliess, killed the seduction theory deader than a doornail.⁸⁹

There were a couple of different sources of evidence that caused Freud to change his mind on the issue of whether or not sexual experiences in childhood inevitably led to neurosis. Freud later credited Havelock Ellis in this regard. Freud claimed that it was his reading of accounts of childhood sexual activity like those described by Ellis in 1903 that finally made him realize that the seduction theory was wrong.⁹⁰ But it clearly wasn’t Ellis who was the original source of

88. Letter of September 21, 1897. In Masson, ed., *Freud–Fliess Letters*, p. 264.

89. Masson, ed., *Freud–Fliess Letters*, pp. 264–266.

90. Sigmund Freud (1905), *Three Essays on the Theory of Sexuality*, Standard

those accounts. It was somebody that Freud apparently did not want to mention. I am convinced that Freud had in mind Albert Moll's book *Untersuchungen über die Libido Sexualis*.⁹¹

PLR: Of 1897?

FJS: Yes. In that book, which Freud read in two unbound parts in the spring and fall of 1897, Moll stated very clearly that, of the numerous kids in German boarding schools who had childhood sexual experiences—Moll had personally conducted a systematic study on this subject—very few later developed perversions or other problems. In Freud's theory, a neurosis is a "repressed perversion," so the same conclusion applies to neuroses. If, for example, one encounters a large sample of people who engaged in homosexual experiences as children, almost none of whom developed perversions or other problems as adults, even fewer of them ought to have developed neuroses, because the number of people who would have subjected these experiences to pathological repression is smaller than the total number. So, on the basis of this new evidence, Freud's seduction theory was suddenly in big trouble.

Felix Gattel, Freud's erstwhile friend and collaborator, was doing some of the same kind of research that Albert Moll was conducting in his systematic interviews with normal individuals. Gattel's research involved conducting clinical interviews in Krafft-Ebing's clinic. Krafft-Ebing was a world-famous psychiatrist who specialized in the problems of sexual perversions. The same difficulty cropped up in the research that Gattel conducted at Krafft-Ebing's clinic: people who weren't hysterics or other kinds of neurotics commonly reported sexual experiences in childhood. It's obvious why Freud later didn't want to credit Albert Moll, by the way. The two were rivals, and Moll later attacked psychoanalysis, so that sealed his fate in the psychoanalytically oriented history books. But I think it was primarily Moll's book, reinforced by Gattel's similar findings, that triggered Freud's final abandonment of the seduction theory. As for Gattel, he and Freud later had a falling out over guess what—the issue of scientific priority. For that reason, Freud was hardly inclined to credit Gattel for having helped to torpedo the seduction theory.

PLR: In a footnote to *Freud, Biologist of the Mind*, you refer to Freud as a birth order "hybrid," whereas in *Born to Rebel* you treat him as

Edition, 7:190–191n. London: Hogarth Press, 1953. See Sulloway, *Freud, Biologist of the Mind*, pp. 312–313.

91. See Footnote 9.

a classic firstborn.⁹² Did your thinking about Freud's birth-order dynamics change?

FJS: Yes.

PLR: Could you explain why? He had two half-brothers and a nephew who were older, though he was the eldest child of his own parents' marriage.

FJS: When I wrote my book on Freud, I was thinking much more along the lines of Freud's own emphasis on the pertinacity of early impressions than I later concluded was justified. It's pretty clear from statistical evidence involving people whose birth-order status changed in the course of their lives that what I call the influence of "functional" birth order is continuous and relatively evenly proportioned throughout childhood. If one goes through half of childhood as a functional firstborn and then becomes a functional laterborn, about half of that portion of one's personality that is shaped by birth order will have been molded by each of the two family niches occupied during childhood.

PLR: How could someone become a functional laterborn?

FJS: It could happen via some disruption of the original family system, which would then make a biological firstborn into a younger sibling, if one's parents remarry and one acquires stepsiblings, for example. The important point is that there's no magic first three years that account for, say, 80% of the variance in personality, with the remaining years of childhood not really mattering all that much. Incidentally, it is because Freud believed in the role of early experience that he emphasized his rivalrous relationship with his nephew John, which lasted until Freud was three years old, when John's family moved to England. When I was first thinking about the possible importance of birth order, I viewed Freud's life more the way he did, in which case the first few years are presumed to count a lot. But in my later research I obtained abundant empirical evidence from people whose functional birth orders changed owing to a disruption to the original family system. This kind of biographical information showed that the first three years didn't count as much as Freud (or I) had thought.

PLR: So if someone becomes a functional firstborn because of sibling loss, that wouldn't be terribly significant? How does your approach weigh the impact of such a potentially traumatic event?

92. Sulloway, *Freud, Biologist of the Mind*, p. 363n2; and *Born to Rebel*, p. 56.

FJS: The crucial issue is, at what age does a person's family niche change? Because the influence of family niches is a continuous developmental process in which each year of childhood counts almost as much as every other year, what one really wants to know is how much of childhood, proportionally, did a given person spend in any given family niche. Freud put a great deal of emphasis on the first three years, so he tended to view himself as a birth-order hybrid. On the basis of my theoretical presuppositions, by the way, I would love to have been able to claim Freud as a psychological laterborn, but he really wasn't.

PLR: And the half-brothers from his father's first marriage?

FJS: The problem with counting them as Freud's elder siblings from a *functional* birth-order perspective is that they were 21 and 23 years older.

PLR: So, in effect, they weren't siblings?

FJS: Right. Freud was functionally a firstborn relative to his own younger siblings. As for Freud's nephew John, there's no evidence that the two of them lived together in the same house. They merely played together on occasion. So Freud appears to have been reading far more into that relationship than was really there. In my computerized data base of the historical evidence used in *Born to Rebel*, which includes more than 6,000 people, I list Freud as the first of six children. In this connection, I followed a formal rule in *Born to Rebel* by which people were coded according to their functional birth order. I was later criticized by one reviewer for not coding everybody according to biological birth order, but that's absurd. In any event, because most people's biological and functional birth orders are the same, that distinction affected such a small percentage of my sample that it made absolutely no difference for the overall results. This distinction affects about 3% or 4% of the sample. Alan Woolf made a big deal about this issue in his review of my book in *The New Republic*.⁹³ He was all worked up about the fact that I was studying not literally *birth* order but, rather, sibling-rearing order. This is the kind of nit-picking sophistry that one sees in a reviewer who wants to be critical about a book but who doesn't really have anything intelligent to say on the subject and so grasps at straws. Had Woolf read the foot-

93. Alan Woolf (1996), "Up from Scientism: What Birth Order and Darwinism Can't Explain." Review of *Born to Rebel* by Frank J. Sulloway, *The New Republic*, December 23, pp. 29–35. See also Frank Sulloway's letter to the editor and Woolf's reply, *The New Republic*, February 3, 1997, pp. 4–5, 48.

notes and appendix to my book, he would have seen the full evidence substantiating the importance of functional birth order and the sometimes misleading value of biological birth order.

By the way, the formal classification rule I followed was that a person had to establish a functional birth order during the first six years of life and had to remain in that functional birth order past the age of 16. If, for example, one grew up as a functional firstborn for the first three years and then became a functional laterborn for the rest of one's childhood, I counted this person as a functional laterborn. But if a person passed his or her seventh birthday and this person's birth order changed, I excluded the person from the sample. I also conducted a separate statistical analysis of such cases in which functional birth order changed and found that my classification rule accorded well with the evidence regarding attitudes in adulthood toward radical changes. In particular, firstborns who were raised as functional laterborns were significantly more likely than other firstborns to support radical social and scientific revolutions.⁹⁴

PLR: So you abandoned the hybrid category?

FJS: Well, I didn't quite abandon it. Instead I refined it through empirical testing. By means of such tests, I was able to show that sibling strategies (for which birth order is merely a proxy) exert a continuous, steady, and cumulative impact on personality throughout the course of early life. This result, by the way, is an effective test of the importance of the first three years of life on personality development. For this and other reasons (for example, the evidence from behavioral genetic studies), I no longer believe in the fundamental importance of the first three years of life the way I once did. Sometimes such seemingly plausible ideas just don't turn out to be true. That's what science is all about.

PLR: Maybe we should let that be the last word. Thanks.

94. Sulloway, *Born to Rebel*, pp. 47, 465–81.