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NOTES ON FLYING AND DYING

BY BERNARD C. MEYER, M.D.

Focused on selected details in the lives and creative works of Samuel Johnson, Edgar Allan Poe, and Houdini, this paper explores a seeming antinomy between claustrophobic annihilation and aviation. At first glance the latter appears as an antidote to the threat of entrapment and death. On a deeper level the distinction fades as the impression arises that in the examples cited, flying may represent an unconscious expression of a wish for death and ultimate reunion.

> Oh I have slipped the surly bonds of earth
> And danced the skies on laughter-silvered wings. . .
>
> John Magee: High Flight

Reminiscent of Icarus escaping from the labyrinth of King Minos on wings of feathers and wax fashioned by his father Daedalus, history and literature provide arresting examples of this same antinomy between entrapment and flying. In Herman Melville's (1846) *Typee*, a novel replete with themes of captivity and enclosures, the narrator and his companion are discovered seemingly hopelessly trapped among the reeds that surround them in their island prison. "I began to think we were fairly ensnared," remarks the narrator, "and had almost made up my mind that without a pair of wings we should never be able to escape" (pp. 53-54). It is not surprising that echoes of this counterpoint are especially conspicuous in those in whose mental makeup claustrophobia or its derivatives play a prominent role. In illustration of this concept I shall discuss selected facets of the lives of three celebrated personages—Samuel Johnson, Edgar Allan Poe, and Houdini—from which I shall endeavor to draw a psychoanalytic meaning.

As in all such explorations of the hidden inner mental life of those no longer living, I shall leave myself open to the charge of faulty methodology, of relying excessively on inadequate or unsupported data, and of formulating hypotheses that can be neither affirmed nor refuted by the voice of the subject. In short, I may be accused of engaging in what has been called wild analysis.

Whatever merit may be contained within these objections, it should be stated without ambiguity that inquiries such as this make no claim to be ranked as a psychoanalysis. Here there is no naïve pretense of placing a departed historical figure "on the couch," or of subjecting him to interpretations as if he were an active participant in the proceedings. On the contrary, like other essays in applied psychoanalysis, the present study seeks to utilize the findings, the experience, and the insights of psychoanalysis as tools capable of lending enrichment to the writing of biography. Addressing himself to the study of history, Herbert Muller (1952) wrote, "The main problem is not so much to fill in the many gaps of our factual knowledge, as to make sense out of the vast deal that we do know" (p. 29). In pursuit of this end, the habitual posture of the psychoanalyst, his skepticism, his vigilance, and his neutrality, may provide an incisive and indispensable tool.

While not granting irrefutable proof of his conclusions, armed with these attributes, the analyst may be in position to discern meanings not commonly visible to the casual eye. In particular, the steady recurrence of certain recognizable themes in the biography of the subject, and the reappearance of these same themes and variations on them in his creative work, can lend to those conclusions a reasonable and persuasive measure of plausibility. This, in turn, may be enhanced by those felicitous instances in which a confirmation of a hypothesis may be discovered in clinical sources.[1]

[1] A twenty-six-year-old man, severely phobic in restaurants, moving vehicles, and other claustra, viewed the psychoanalytic situation as a latter-day version of

SAMUEL JOHNSON

During the latter part of 1784, the last year of his life, Johnson's health underwent a swift decline. Although he had been ailing for some time, specifically with symptoms of congestive heart failure, his condition took an abrupt turn for the worse on the heels of a most painful blow. In July of that year he learned to his dismay of rumors of the impending marriage of his beloved Hester Thrale to Gabriel Piozzi, an Italian musician, with whom she had fallen deeply in love following the death of her husband early in 1781. Although it is unknown whether Johnson had ever seriously considered marriage to her, it is evident that he perceived her action as a cruel rejection of him whom she had mothered and sheltered during the preceding eighteen years. It had been the warmth and hospitality of the Thrales that had lifted him out of a black depression in 1766, and from that time on they had provided him with a home and a family life that he had virtually never known before. Clearly flattered to play hostess to so remarkable and eminent a personage, Hester made her company available for conversation at all hours, and, like a good mother, although she was young enough to be his daughter, treated him like an overgrown baby, sitting up with him as late as three and four in the morning, while catering to his seemingly insatiable thirst for tea. It was a craving for which his earlier history quite naturally paved the way.

Born after a difficult and dangerous labor to his forty-year-old

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the rack, the wheel, and other accoutrements of the Inquisition. It became evident that these reactions were related to a dimly recalled surgical procedure performed when the patient was about six. In view of his fear of passivity, it is not surprising that he was subject to a severe sleep disturbance, which, since the beginning of his analysis, occurred with striking regularity on the eve of his appointments. Because of the danger of a loss of control, he spurned the use of hypnotics, save in ineffective homeopathic dosage. At times, however, he succeeded in combatting his insomnia by reciting little jingles he had heard on the television. Of these his favorite was: "Up, up and away on TWA."

mother, Sam Johnson was deemed so frail that his parents, fearing he would not survive, had him christened on the same day. Although this fear proved groundless, new crises were not long in coming, for he was placed in the care of a wet nurse who, unknowingly, was afflicted with tuberculosis. From her little Sam contracted scrofula, a tuberculous involvement of the lymph nodes of the neck. These were incised, presumably more than once, and, in keeping with the state of surgery of that day, without anesthesia. This procedure, and an "issue" cut into his arm to "drain the infection," did not prevent the formation of defects in his sight and hearing. Moreover, there is reason to believe that these operations played a role in the genesis of the frightful tics and convulsive movements that would plague him throughout his life.

Although at the time of his mother's death he declared that she had been "the best woman in the world," neither his behavior toward her, nor his writings quite harmonized with that sentiment. Nor did he retain fond memories of her treatment of him during childhood. "Sunday was a heavy day for me when I was a boy," he told Boswell (1791). "My mother confined me on that day and made me read [a book entitled] *The Whole Duty of Man*" (p. 50). Perhaps it was to escape her domination and infringements on his liberty that prompted his neglect of her after he left his native Lichfield at the age of twenty-seven. For, except for one or two visits made not long after his departure, during the remaining twenty-two years of her life he never saw her again. All this time, it should be noted, he was living in London, a mere one hundred and twenty miles away. However, with the approach of her death he began to make amiable overtures to her, and he spoke of going to Lichfield. But this he did not do until she died, when he began to make an almost annual pilgrimage to his native town.

In this, his behavior was typical of what he displayed in other relationships. A counterpart of the physical distance he maintained from his mother was the remoteness of age separating him from others. His marriage at twenty-five to a widow of

forty-six serves as an example of this avoidance of his contemporaries, as is his relationship with Hester Thrale, who was more than thirty years his junior, and with James Boswell, who was even younger. As with his mother, he often delayed expressions of intimacy and tenderness until the objects of those sentiments were drawing away or dying. Thus, after his mother's death he made the perplexing comment, "The life which made my own life pleasant is at an end and the gates of death are shut upon my prospects" (Bate, 1963, pp. 128-129). And when he was faced with the loss of Hester Thrale to Gabriel Piozzi, his world virtually fell apart.

"If you have abandoned your children and your religion," he wrote to her, "God forgive your wickedness; if you have forfeited your Fame, and your country, may your folly do no further mischief. If the last act is yet to do," he continued, hoping she was not yet married, "I, who have loved you, esteemed you, reverenced you, and *served you,* I who long thought you the first of human kind, entreat that before your fate is irrevocable, I may once more see you. I was, I once was, madam, most truly yours, Sam Johnson" (Johnson, 1783-1784, p. 174, italics added). On 23 July 1784 Hester and Mr. Piozzi were married.

What prompts a person to become ill, it has been said, to "enter the preterminal phase, and die at a particular time and in a particular way cannot always be ascribed to a disease process. The final illness is a psychosocial as well as medical event, in the same way that a person's complete biography is something more than the sum total of medical and nursing notes made during his lifetime" (Weisman and Kastenbaum, 1968, p. 1). Although Johnson continued to live for another five months after Hester's marriage, he became increasingly incapacitated by heart failure, manifested notably by severe breathlessness and edema of the extremities. To these ominous portents Johnson responded with a mixture of surrender and defiance. "Let me alone; let me alone; I am overpowered," he cried out a month before his death. As it drew nearer he de-

clared, "I will be conquered; I will not capitulate" (Bate, 1977, p. 597). Indeed his intransigence in the face of death will soon be related.

Typical of the enduring vitality of his spirit and his ceaseless involvement in the adventures of the human mind was the fact that, even as death threatened to overtake him, he joined in the general enthusiasm for the rapidly growing advances in balloon aviation that were taking place on both sides of the English Channel during the final year of his life. His letters during these months resound with this counterpoint between flying and dying.

"My legs and my thighs grow very tumid," he wrote Hester early in the year, but a few paragraphs later he spoke of the balloon which "engages all Mankind, and . . . is indeed a wonderful and unexpected addition to human knowledge" (Johnson, 1783-1784, p. 123). And to her daughter, Queenie, although he lamented the steady decline of his health and his growing despair, he was unable to repress his excitement over the balloons, assuring her of his continuing "connection with the world so far as to subscribe to a new ballon [*sic*] . . . by which, I suppose, some Americo Vespucci . . . will bring us what intelligence he can gather in the clouds" (p. 128). (Many of the balloon flights of that day were financed by public subscription.) Less than a month before his death he sent his faithful servant, Francis Barber, to Oxford to watch a balloon ascension, regretting that the state of his health prevented his presence there.

Lest it be assumed that Johnson's fascination with flying in 1784 was nothing more than an expression of his susceptibility to the general excitement that prevailed at that time over balloon aviation and bore no individual significance, it should be pointed out that this was not the first manifestation of his interest in flying. "A Dissertation on the Art of Flying" is the title of Chapter VI of *Rasselas* (1759), that fictional work which Johnson composed at the time of his mother's death. Although it has been claimed that it was written to defray the expenses

of her funeral, it appears more likely that the sudden access of energy culminating in its swift composition—allegedly in the course of eight consecutive nights—was for Johnson, as it has been for so many creative persons, a reaction to a painful and threatening loss, a process that has been likened to a sort of substitute mourning.

Pervaded by a mood of melancholy—"human life is everywhere a state in which much is to be endured and little to be enjoyed" (p. 65)—*Rasselas* is a tale of tombs and catacombs, of grief and "the pangs of separation," of madness, and of death. Interwoven with these themes are observations on the practice of embalming, on the fear of ghosts, and on the matter of communication with the dead. The story is named for an Abyssinian prince who is compelled to live in a palace situated in a spacious valley which, cut off from the rest of the world, is surrounded by lofty and impassable mountains. Known as "The Happy Valley," this terrestrial claustrum can be entered only through a secret passage traversing a cavern whose outlet is concealed by a thick wood, and whose mouth, opening into the valley, is closed with gates of iron. From the mountains on all sides are "rivulets that descended, filling all the valley with verdure and fertility, and formed a lake in the middle, which discharged its superfluities by a stream which entered a dark cleft . . . and fell with dreadful noise . . . till it was heard no more . . . . every blast shook spices from the rocks, and *every month dropped fruits upon the ground*" (pp. 39-40, italics added).

Despite the seeming contentment of most of the inhabitants, the prince is afflicted by an insufferable boredom in his prison of "tasteless tranquility," and in the course of his twenty-sixth year—virtually the same age when Johnson left his mother in Lichfield—he begins to plan his escape. At one point, as narrated in Chapter VI, he is encouraged by a resourceful inventor into believing that he may attain his freedom by flying. "He that can swim," he tells Rasselas, "must not despair to fly . . . to swim is to fly in a grosser fluid, and to fly is to swim in a subtler" (p. 51). Unfortunately, despite his confidence that one

day he would take to the air and leave eagles and vultures behind, when the day arrives for his maiden flight, his pinions fail to sustain him, and he plunges ignominiously into the lake below.

Somewhat later, Rasselas, together with his philosopher friend Imlac, succeeds in digging a tunnel under the mountain and, accompanied by his sister Nekaya and her companion Pekuah, gains access to the outer world. But here, alas, all is misery and melancholy. Ironically, the travelers who have escaped the encircling confines of the Happy Valley are soon confronted by a show of claustrophobia when, urged to explore the interior of an Egyptian pyramid, Pekuah is seized by an unendurable fright. Asked of what she is afraid, she replies, "Of the narrow entrance . . . and of the dreadful gloom. I dare not enter a place which must surely be inhabited by unquiet souls . . . . if the princess is pleased that her servant should die, let her command some death less dreadful than enclosure in this horrid cavern . . . . if I once enter, I never shall come back" (p. 107). Taking pity on the panic-stricken girl, the princess allows her to remain outside, but this only invites a new danger, for, while the rest of the party explores the interior of the pyramid, Pekuah is abducted by a band of Arabs. Sunk in grief, the princess torments herself for having abandoned her unhappy servant to a miserable fate. But in time Pekuah's whereabouts are discovered, and, after paying a ransom to her captors, her mistress secures her release. Ultimately, the travelers return to the Happy Valley, their "prison of pleasure."

In light of the explicit allusions to death, to enclosures, and to the gynecomorphous symbolism of the Happy Valley—there is a cemetery by that name in Hong Kong—there are compelling reasons to suspect that the themes of claustrophobia and death, so recurrently sounded in this work, were echoes of those same sad tunes inspired by the death of Johnson's mother. But this would not be the only occasion when these themes found expression in his writing.

Some years later, in 1766, early in his acquaintanceship with the Thrales, he composed "The Fountains," a fairy tale, in which again the themes of flying and incarceration are prominently depicted, on this occasion in the form of birds and cages. It is the story of the rescue by the girl Floretta of a lovely goldfinch, which, entangled by a lime twig, is menaced by a hawk poised in readiness to seize the hapless creature in its talons. Undismayed by the danger, Floretta boldly frees the bird and places it securely in her bosom. Written not many months before his rescue by the Thrales from the depths of despair, it is not surprising that Floretta, as Johnson acknowledged, was modeled after Hester. Yet, if the story is to be read as a wishful anticipation of his own rescue, what follows suggests that his hopeful expectations were not unmixed with distrust, for no sooner does Floretta show the bird to her mother than the latter proposes that "it would make a very proper inhabitant of the little gilded cage which had hung empty since its previous tenant, a starling, had died there for want of water" (Johnson, 1766, p. 10). Horrified lest her beloved goldfinch suffer a like fate, Floretta, defying her heartless mother, puts the bird's bill to her lips and sets him free. There was something prophetic in the note of distrust sounded in this tale, for, as I noted earlier, although Hester had released him from the snares of melancholy, the time would come when Johnson would feel that she had rejected him and flung him, heart-broken, into the merciless embrace of death.

Even prior to these literary excursions into the subject of flying, however, Johnson had given evidence of his fascination with that theme. As early as 1753, in an article in *The Adventurer,* he had chided those who ridicule inventors, citing in particular those who "would consider a flight through the air in a winged chariot . . . dreams of mechanik lunacy" (Bate, 1963, p. 434). These comments, as well as the "digression" in *Rasselas,* are noteworthy not merely as a reflection of the scope of Johnson's scientific curiosity, but for his remarkably prescient recognition of the ultimate consequences of man's poten-

tial conquest of the air. "If men were all virtuous," observes the inventor in *Rasselas*, "I would with alacrity teach them all to fly. But what would be the security of the good if the bad could at pleasure invade them from the sky?" (Johnson, 1759, p. 52). Anticipating modern air warfare, he declares, "Against an army sailing through the clouds, neither walls nor mountains nor seas could afford any security" (p. 52).

It was not only in his writing, however, that Johnson revealed his more than passing concern with confinement and escape. In recent years interest has been focused on a padlock which he entrusted to the care and secrecy of Hester Thrale a year or so after the Thrales had welcomed him into their home. There is some reason to believe, moreover, that more than a single padlock was involved, for in writing of this mystery in her diary, Hester stated, without any elaboration, that "the Fetters and Padlocks will tell Posterity the truth" (Thrale, 1776-1809, p. 415). How that truth has been interpreted, however, has not been uniform. One biographer, W. Jackson Bate (1977), noting that in those days the insane were often kept in chains, has argued that Johnson's possession of padlocks and fetters was simply an expression of his recurring fear of madness, and a protection against it. There is no question that Johnson was much concerned with, and feared, insanity. That classic of psychiatry, Burton's *Anatomy of Melancholy*, was the only book, Johnson declared, that ever took him out of bed two hours sooner than he intended to rise. It is also known that he made several trips to "Bedlam" to see the chained madmen. Yet it is difficult to see how these facts lend any support to Bate's hypothesis that Johnson "bought the fetters and padlocks lest the enemy that seemed to be winning against him pass beyond control" (p. 385). Johnson's shackles offered him no greater immunity, it would appear, against the threat of insanity than would a pair of crutches serve to ward off the threat, say, of a stroke or poliomyelitis.

Far more plausible is the hypothesis, advanced by Katherine Balderston (1949), that these objects served as instruments in

Johnson's disordered sexual life, a theory which, to the clinician, suggests the practice of bondage. In support of her argument she cited (pp. 5-6) an entry in Johnson's diary for 1771, written in Latin (as he often did in matters of privacy), reading, "*De pedicis et manicis insana cogitatio*" (Insane thought about foot fetters and manacles).

Further evidence that he courted conditions of enforced immobilization is supplied by a letter, written again in a foreign language—French, this time—which was sent by him to Mrs. Thrale while both of them were living under the same roof in 1773. Chafing under a painful sense of abandonment occasioned by her preoccupation with her dying mother, Johnson made the bizarre request that he be subjected to a code of rules and regulations governing his movements on the premises. He asked specifically to be instructed whether he was to be confined to "prescribed limits" within the house, and to be told what "is permitted and what is forbidden" (Balderston, 1949, pp. 6-7). If Mrs. Thrale desires to prevent him from wandering at will about the house, he continued, she has only to take the trouble of turning the key to his room twice a day and thus confine him there. (Shades of *The Whole Duty of Man*!) In closing, he voices the hope that as his master (*patronne*) she will continue to exercise her authority over him and subject him to that condition of enslavement (*esclavage*) which she knows so well to render pleasing.

Replying in English to this extraordinary letter, characterized by Balderston (1949, p. 8) as "a pathological document, the product of a sick mind," Hester chided him for "brooding in secret on an Idea hateful in itself," and urged him not to "allow his fancy thus to dwell on Confinement and Severity. . . . So farewell and be good," she concluded, "and do not quarrel with your Governess for not using the Rod enough" (p. 7).

As noted earlier, her marriage to Signor Piozzi, alas, brought to an end this claustrophilic idyll, and, like the caged starling in "The Fountains," which had perished in its gilded cage for

want of water, Johnson, too, in the space of but a few months, and bereft of the encircling solicitude of his "governess," died while scanning the skies and discerning in man's incipient conquest of the air a compelling reflection of his own indomitable spirit.

EDGAR ALLAN POE

In this interplay between dying and flying, enacted in the setting of a claustrum, Johnson was anticipating a fictional pairing of these opposing themes in the writings of Poe. Like Johnson's, Poe's infancy had been scarred by tuberculosis, that "great white plague" that had made him an orphan in his third year, not long after his stricken mother had given birth to a daughter. All his life, it is said, he carried with him a portrait locket which his mother had left him, a physical emblem of that union of love and death that permeated his life and fiction.

After the death from tuberculosis of Virginia, his child wife, he often wandered to her grave at night, calling upon her "in words of devoted love." In later years he is said to have proposed marriage to another woman in a cemetery (Wagenknecht, 1963), and from yet another he exacted the promise that she would come to him after he took his own life. Like Johnson, he could not endure solitude, especially at night when he would cause his mother-in-law (like Hester Thrale) to sit up with him until the early hours of the morning, while he sat at his desk writing, and she sat dozing in a chair. "For your sake," he once told her, "it would be sweet to live, but we must die together" (Wagenknecht, 1963, p. 53).

> I could not love except where Death
> Was mingling his with Beauty's breath—
> Or Hymen, Time and Destiny,
> Were stalking between her and me (p. 227).

As a means of warding off such morbid thoughts and his fear of insanity, in his fiction from time to time Poe turned toward

tales which celebrated the triumph of cold reason. He took refuge in ciphers and codes and in the detective story, like "The Gold Bug," in which the employment of the brain as a thinking machine was designed to defend him against the danger of the disintegration of the mind through madness.

As an alternative he combatted his frightening fantasies of becoming engulfed within the lethal embrace of a mother's love by assuming a posture of active cruelty which he displayed both in his personal life and in his tales. Reminiscent of the mother of Johnson's Floretta, who sought to imprison the pretty goldfinch, on one occasion Poe somehow caught a wild bobolink and confined it in a cage. "The poor bird was as unfit to live in a cage as his captor was to live in the world," wrote one biographer (Stoddard, 1895, p. 137). The bird was "as restless as his jailer, and sprang continually in a fierce, frightened way from one side of the cage to the other. I pitied him, but Poe was bent on training him. There he stood, with his arms crossed, before the tormented bird" (p. 137). Here, in his personal life, Poe revealed that fascination with incarceration and cruelty so insistently depicted in his fiction.

In his excellent paper, "Claustrophobia and Depression," Asch (1966) has referred to Poe and especially to his "Pit and the Pendulum" as compelling illustrations, drawn from literature, of his clinical thesis. Yet from time to time, Poe, that poet laureate of claustrophobia, came up for air, as it were, forsaking his grim tales of living burials and ghastly entombments to write stories about balloon travel, in which the morbid mood of "The Premature Burial," "The Black Cat," "The Cask of Amontillado," and other such tales was replaced and relieved by a near manic lighter-than-air euphoria. Nothing could better illustrate the antidotal thrust of these stories about flying than the comment of the narrator of "The Unparalleled Adventures of One Hans Pfaall" (note the allusion to disaster in the pun, shallowly buried in the hero's name) that he had undertaken his aerial voyage *as an alternative to committing suicide*. But, seen from the point of view of Poe's habitual posture both in

his life and in his fiction, this was not the only contradiction contained within the tale.

Prior to embarking upon his lunar flight the balloonist has brought on board a pair of pigeons and a cat. Later in the course of the voyage, and much to his astonishment, the cat gives birth to three kittens, a striking affirmation of life by an author whose thought and fame is so inexorably linked with sterility and death. Indeed, this detail stands in stark contrast to that tale of murder, "The Black Cat," in which cruelty to cats plays a central role. It should be noted in passing that with one exception none of Poe's fictional women bears a child. His own wife remained childless during the eleven years of their marriage which took place when she was but thirteen years of age. What maternal feelings she displayed were lavished on Caterina, a cat which the unhappy Virginia clutched to her bosom as she lay in her bed, dying slowly of consumption. When the narrator of "The Black Cat," awaiting execution for the murder of his wife, tells how his crime had been revealed by the wailing of her cat that he had inadvertently walled up with the body of his wife, he remarks that the cry sounded very "like the sobbing of a child."

It was not only the content of the Hans Pfaall story, however, which served as an antidote to the morbid atmosphere of Poe's deadly claustrophobic tales. His very behavior while writing it reflected a dramatic reversal of his habitual melancholy. A visitor at that time said that Poe, in describing the story, "became very excited, spoke rapidly, 'gesticulating much,' and finally 'clapped his hands and stamped his foot by way of emphasis. . . . When he had finished his description he apologized for his excitability, which he laughed at himself' " (Wagenknecht, 1963, p. 45). One critic spoke of "balloonacy." [2]

[2] Lewin (1953) reported that one of Kubie's patients described an experience during which he hallucinated an all-enveloping wall, endless and pure white, a dream which "was the worst nightmare of his childhood" (p. 180). Significantly, the patient also had "overactive, self-confident dreams of flight," which Lewin interpreted as a manic type of escape.

Further evidence of his fascination with aerial travel is provided by the story he concocted in 1844 of the successful crossing of the Atlantic Ocean by a balloon, named the "Victoria." So convincing was the writing that it was published as an authentic news item on April 13 in the *New York Sun*, many of whose readers believed it to be true. Reminiscent of Johnson's prophetic observations on the future of aviation, Poe's account, later published in "The Balloon Hoax," asserted that "The air, as well as the earth and the ocean, has been subdued by science, and will become a common and convenient highway for mankind" (Poe, N.D., Vol. I, Part II, p. 143).

Poe's flights into fancy and mania, however, did not keep him psychologically aloft, nor did they serve as a substitute for his drinking, which was also abetted by his need to escape from sick fancies. "As a matter of course," he said, "my enemies referred the insanity to the drink, rather than the drink to the insanity" (Wagenknecht, 1963, p. 39). Despite his repeated resolutions to abjure liquor, he was unable to withstand the least stress without resorting to it. So sensitive was he to alcohol, it was claimed, that a single glass of wine could make him "hopelessly drunk." Exaggerated or not, there is little doubt that drinking was the cause of his death, although the circumstances surrounding it have been a matter of some controversy. Of particular interest, because of its relevance to a characteristic of Houdini, is the fact that Poe's final bout with alcohol occurred in a setting of travel, namely, in relation to a railroad journey from Baltimore to Philadelphia. Since in those days there was no bridge across the Susquehanna River at Havre de Grace, train passengers were obliged to disembark and make the crossing by ferry. According to one source, because of his fear of the rough currents on the river, Poe refused to board the boat and returned instead to Baltimore where, afflicted by a fit of delirium tremens, he was admitted to a hospital where he died (Moran, 1885).

HOUDINI

As a virtuoso performer—"Don't insult me by calling me a magician," he insisted, "I am an escape artist" (Meyer, 1976, p. xi)—it was quite natural that Houdini was drawn to the writings of Poe. One of Houdini's most prized possessions was Poe's portable writing desk, and among the very few pictures retained by this ardent necrologist of the graves of persons who were neither family members, nor friends, nor fellow magicians was a photograph of the tombstone in Baltimore of Edgar Allan Poe. Yet beyond these indications of the spiritual kinship he felt with Poe was his announcement at the outset of his movie-making career that "Edgar Allan Poe will furnish the first scenarios, as his tales contain the desired amount of mysticism, danger and opportunity for physical exertion" (p. 172). It is curious that in thus characterizing Poe's writing, he omitted its most arresting commonality with his owns interests: an unceasing fascination with the macabre, with living burials, with claustra, suffocation, death, cemeteries, and heaven-sent escape. Such an omission, one may suspect, was dictated by Houdini's eagerness to disavow anything morbid in his own artistry. Yet what other adjective may be appropriately affixed to his willingness to be sewn into the belly of the carcass of a so-called sea monster, to be tied at the stake while surrounded by a flaming ring of burning faggots, or to be buried under six feet of earth? Like the narrator of Poe's "Premature Burial," who, believing he had been accidentally nailed alive into a coffin, wrote of his "despair—such as no other species of wretchedness ever calls into being," Houdini, buried deep in the earth, "tried to shout, merely wasting precious breath and choking his mouth and nose with sand." "I tried out *Buried Alive*," he wrote, "and nearly did it. Very dangerous; the weight of the earth is killing" (Christopher, 1970, p. 146). A charismatic showman, upon the stages of innumerable theaters in America and abroad he enacted a varied repertoire of seemingly hairbreadth escapes. He was the inspired choreographer of the melancholy strains of

Poe's narratives, a gifted mime, whose magical gestures illumined the poet's chilling visions.

It should come as no surprise to discover that the celebrated escape artist was in more than one sense a "closet" claustrophobe. Once, relatively early in his career after he had entered a telephone "closet" in the lobby of a Kansas City hotel, a prankster passing by turned the key and locked the door from the outside. When the Champion Jail Breaker discovered that he could not get out, he broke into a terrifying rage, hammering with his fists on the door, kicking and screaming as if he were imprisoned for life. "What fool trick is this!," he shouted. From this time on, it is said, he never entered a telephone booth without placing his foot securely in the path of the door. As an added precaution, he always took care to carry in his pocket a little "gimmick," about the size of a pocket knife, that contained a set of steel lock picks which could open most doors. Despite these safeguards, some years later he was caught again in the same trap when, at a magician's banquet, a group of confreres decided to imprison the World Famous Self-Liberator in a pay toilet. As on the previous occasion, Houdini failed to find anything funny in the situation and unleashed a storm of fury at his would-be captors.

There is reason to suspect, moreover, that his notorious motion sickness at sea was a derivative of claustrophobia. This recalls Poe's reputed fear of crossing the Susquehanna River just prior to his final alcoholic binge. It was asserted that for Houdini the mere act of buying steamship tickets while still on dry land was enough to cause him to turn green and become violently nauseated. A nightmare of seasickness, relieved only by a quiet passage through the Suez Canal, occurred during a twenty-nine-day voyage from Marseilles to Melbourne, Australia, in the course of which he ate only fourteen meals and lost twenty-eight pounds. What was of no less importance on this voyage is the fact that crated within the hold of the ship was an airplane which Houdini had just recently purchased, and which was to mark a new and exciting chapter in his life.

It began in 1909, a year that was marked by a particularly strong upsurge in enthusiasm for aviation. In July of the previous year the celebrated American pioneer aviator, Wilbur Wright, had come to France to set up shop at Le Mans. His subsequent flights created a sensation as he promptly shattered all established flying records. In October 1908 he kept a plane aloft for over an hour. Shortly before Christmas he succeeded in covering ninety-nine kilometers in one hour and fifty-three minutes, climbing to a height of forty-six feet. These records were soon eclipsed by others.

Toward the beginning of November 1909, while performing in Hamburg, Germany, Houdini joined a throng and went out to the racetrack where he saw his first flight. He was beside himself with excitement, and when the plane landed safely after a brief and erratic spin, Houdini rushed up to the pilot and besieged him with questions, demanding to know how he might learn to fly, and how he might acquire a plane for himself. Within a week he became the owner of a French Voisin plane, and he rented a wooden house in which to store it. From now on the plane became his obsession; in his diary virtually all other subjects disappeared. "Everything was flying," wrote one biographer. "Each morning early he dashed out to his new toy" (Kellock, 1928, p. 214). Characteristically, he had the name HOUDINI painted on the sides of the aircraft and had his picture taken standing beside it—his cap on backwards, in proper aviator fashion. Finally, on November 26, after a delay occasioned by bad weather, and an accident in which he smashed the propeller, he made his first successful flight. On January 7, accompanied by his flying instructor, he embarked for Australia, where he was scheduled to give numerous stage performances in several major cities. Until the time of his arrival no one had yet made a successful flight in Australia. When Houdini discovered that he stood a chance of being the first to do so, he could think of little else. Every night after his show in Melbourne, he left the city to spend the night with his plane at a field some twenty miles away.

Once again his eagerness to fly was hindered by bad weather, but finally, on the 16th of March, 1910, at five in the morning he took off, circled the field, and, amid a wildly cheering crowd, made a perfect landing. Thus it was that Houdini, the Champion Jail Breaker, became the first person to fly an airplane in Australia. On that day he made three more trips, remaining in the air on one occasion for nearly five minutes. Flying at an altitude of one hundred feet, he covered an estimated distance of seven miles.[3]

When his show opened in Sydney, he lost little time in securing a flying field. Here again he would get up as early as two in the morning to be with his plane. After four successful flights, however, he crashed, and although he himself was unhurt, the plane was badly damaged. He was described as "a wreck," evidently as a consequence of the strain of seeking to pursue both his theatrical and his flying careers at the same time. He acknowledged that he had not had much sleep for two months, having "lost the habit," he supposed. By the time he embarked for Vancouver on May 11 his zest for flying seemed to have left him. His passionate fling with aviation came to an end, and the excitement and euphoria generated by his conquest of the air gave way to the gloomy prospect of unceasing nausea within the suffocating confines of the ship. "I have seen the steamer," remarked the aeronaut before his departure, "and I'm sick already" (Christopher, 1970, p. 123).

Despite this colorful show of the opposing polarities of flying and dying, Houdini's early history, unlike Poe's, fails to account for his preoccupation with those themes. On the contrary, the story of his early years is distinguished by its unreliability and its omissions, in which authentic data are replaced by myth-like fabrications, invented largely by a mother who tinkered with truth and fiddled with facts with a virtuosity that bespoke a rich and fertile imagination. Seeking to provide her son with the sense of security implicit in being of American birth, she

[3] It is curious to learn that although Houdini once bought an automobile, he seldom drove it, claiming that driving made him nervous.

let it be believed that, like his younger siblings, he was a native of Appleton, Wisconsin, where his father, Rabbi Mayer Samuel Weiss, was the leader of a small congregation.

But in truth, Ehrich, for such was Houdini's given name, was born in Budapest, as were some older brothers. In bypassing this important detail, his mother succeeded in obscuring from view a crucial chapter in the history of the future escape king, a chapter which should have narrated the traumatic upheaval of a family, which, uprooted from its native home, was subjected to a painful migration halfway across Europe, across the seas, and again halfway across America. In the absence of any account of this hegira, it is tempting to fill in the blanks that might lead to an understanding of the evolution of Houdini's subsequent life and mental makeup. It is difficult to resist the speculation, for example, that his later seasickness may have been related to the chaos and anxiety occasioned by his early trans-Atlantic passage. By the same token, in view of his later obsession with "graves and worms and epitaphs," it may be suspected that, as with Poe, those early years were punctuated with the threat of annihilation and the show of death.

Instead, the early history of Ehrich Weiss is garnished by what Dr. Johnson once called luscious falsehoods, and which served to provide Houdini with rich nourishment for the establishment of a glamorous family romance fantasy. So great was his father's celebrity, it was claimed, that he was the object of the homage of the "Kaiserin Josephine," who used to visit him from time to time in order to pay her respects. Unfortunately, the history of the Austro-Hungarian empire and its genealogy of royalty fails to mention any sovereign of that name. No more plausible, though equally attractive, is the assertion that the rabbi had been forced to flee Hungary in order to escape the wrathful vengeance of the family of a nobleman whom he had slain in a duel of honor. No more authentic, furthermore, was the contention that Ehrich had been named for another baby bearing the same name, who had died suddenly after a fall, a canard invented evidently to support the myth of Houdini's American birth.

Where fancy leaves off and fact emerges in the life history of Houdini, and where it begins to show some correspondence with the morbid events in the life of Poe, is the appearance of tuberculosis in the Weiss household. When Ehrich was not yet twelve years old, his twenty-two-year-old half-brother Herman was stricken with a rapidly advancing attack of that disease. Just short of six weeks after his admission to a hospital he died. Perhaps because of this misfortune, Rabbi Weiss began to entertain gloomy forebodings about his own death, and a few months later, on the occasion of Ehrich's twelfth birthday, he supposedly summoned the boy to his side and bade him promise to look after his mother as long as she lived.

Hardly in keeping with this promise, on the following day, anticipating, it may be, his later obsession with escape from the jaws of death, Ehrich ran away. Some time later his mother received a postcard, addressed pointedly to her, announcing his presumed destination and his intention to return home within a year. Bearing the salutation "Dear Ma," it was signed "Your truant son." In thus singling her out as the object of his flight, he provided a foretaste of the devotion he would extend to her in the years to come.

Since she was the central focus of his emotional life, even after his marriage, it is not surprising that her death at the age of seventy-two came as a shattering blow. He was performing in Germany at the time, and the news supposedly caused him to fall unconscious to the floor. He promptly canceled the rest of his tour and returned to America where the funeral had been delayed so that he might have a last look at her. Recalling Poe's nightly visits to Virginia's grave, Houdini made frequent visits to the cemetery bearing his mother's remains, where he would lie face down on the grave and speak to her. Along Broadway it was rumored that grief had driven him insane.

As Poe (N.D., Vol. III, Part III, p. 32) had written

> Because I feel that in the heavens above
> The angels, whispering to one another,
> Can find among their burning terms of love
> None so devotional as that of Mother,

in the dedication of one of his books, Houdini (1924) declared

> If God in His Infinite Wisdom
> Ever sent an angel upon earth in human
> Form, it was my Mother.

Like Poe, and like Johnson too, Houdini remained childless. While Poe's child bride played with other children and her cat, Bess Houdini played with dolls and doll houses and lavished her maternal affection on pets. Recalling the goldfinch threatened with imprisonment in a cage in Johnson's fairy tale, "The Fountains," Poe's cruel incarceration of the bobolink, and the pigeons carried in Hans Pfaall's balloon, in the Houdini household much affection was given to canaries. When one of them died, Houdini did not fail to mention it in his diary, that steady repository of recorded deaths and disasters. "Poor birdy," he wrote, "and poor Bess." Especially relevant to the present essay, in which the themes of dying and flying are linked with claustra and mothering, is the message sent to Harry Kellar, a fellow magician, announcing the birth of a canary: "*As it broke out of its shell,*" wrote the escape artist, "*we called it Houdini*" (*Diary,* unpublished). Surely it was not without significance that Kellar referred to Houdini's airplane as his "toy" and "pet."

From the foregoing vignettes it is clear that for these persons, at least, the appeal of flying answered more than a yearning for freedom. Indeed, the flier may develop a special emotional attachment for his aircraft that may supplant human relationships. In his observation of air force pilots during World War II, Bond (1952) described comments by airmen in support of this finding. "After flying, my wife comes first," remarked one pilot, while another declared that "No woman will ever come between me and flying" (pp. 21, 26).

There is evidence too that for some fliers the instrument of flight is viewed as a part of the self, an extension of identity endowed with magical and unearthly attributes. In his novel, *Night Flight,* the aviator-writer Antoine de Saint-Exupéry (1932) spoke of this union between flier and plane—"of the

mystery of metal turned into living flesh." When Fabien, the doomed hero of that book, touched the steel rib of the plane, "he could feel the stream of life flowing into it . . . it was alive" (p. 21). Speaking of that unity, the author once told his mother, "You have no idea of the calm, the solitude, that one finds in a tête-à-tête with one's engine at an altitude of 4000 meters" (1955, p. 97).

But like Icarus who fell to earth because he flew too near the sun, and like Fabien, Saint-Exupéry met his death while flying. Neither his body nor his plane was ever found, nor has any cause been discovered for his disappearance while he was on a reconnaissance mission over his native land during World War II. There is a kind of poignant irony in the realization that his final flight carried him over that part of France he knew and loved best—those places that contained the treasured landmarks of his childhood and the sweet memories of his widowed mother.

"What taught me the notion of immensity," he once wrote to her, "was not the Milky Way, nor flying, nor the sea, but the second bed in your room. It was marvelous luck to be sick. Each of us yearned to be ill in turn. It was a limitless ocean to which we were admitted by getting the 'flu' " (1955, p. 161). Metaphorically, it may be said that he regained that blissful haven when he vanished into the vast uncharted skies that blanketed those distant scenes.

A comparable reunion with things past may be discerned in the final chapters in the lives of Johnson, Poe, and Houdini. On the eve of his death Johnson re-enacted the traumatic events that had ushered in the earliest period of his life. When the fluid accumulating in his swollen legs failed to respond to the physicians' remedies, he demanded that his surgeon make incisions in his calves, and when he thought them too superficial he decided to take matters into his own hands. When no one was looking, he seized a pair of scissors and plunged them deeply into the back of each leg. On the following day he died. With these self-inflicted wounds, the mortification of the flesh

that had been imposed upon him as an innocent child came full circle, and, as in a well-contrived sonata, the coda of his life resounded with the sad strains of the original melody.

During the last year of Poe's life, when that perilous mental equilibrium became undone, giving way to periods of grandiosity and delusions of persecution, he made a suicidal gesture under circumstances which were arresting. To Annie, a member of that parade of women with whom he fancied himself desperately in love, he wrote that while he had been in Providence, Rhode Island, he had procured two ounces of laudanum with which he intended to take his life. Then he took the train to Boston where he begged her to join him, reminding her of "that holy promise which was the last I exacted from you . . . that you would come to me on my bed of death" (Krutch, 1926, p. 179). That this *Liebestod* was to take place in Boston is noteworthy, for it was the city of his birth. On the reverse of the miniature portrait presented to him by his mother was an inscription urging him to "love Boston, the place of his birth and where his mother found the best and most sympathetic friends" (p. 138). Thus it would appear that symbolically his intended suicide was designed to effect a reunion with her, a rebirth, as it were, enacted on his "bed of death." [4]

Similar sentiments are implicit in Houdini's will. "My body is to be embalmed and buried in the same manner in which my beloved mother was buried upon her death, and my grave [is to] be constructed in the same manner as my beloved mother's last resting place was constructed for her burial, and I also direct that *I shall be buried in the grave immediately alongside that of my dear departed mother*" (italics added).

In view of the ultimate fate of these several personages, it

[4] Nevertheless, Poe's sentiments toward his native city were far from consistent. Though his first volume of poems bore upon its title page the legend, "By a Bostonian," his subsequent allusions to that city were filled with bitterness and hatred. Although several explanations have been advanced for what one biographer termed Poe's "blind fury" toward the "hub of American culture," it is difficult to avoid the suspicion that ultimately it represented the underside of the ambivalence he felt toward the mother who abandoned him by her untimely death.

might be reasoned that their fascination with aviation served not merely as a figurative barrier against the frightful menace of death, but as a means of sweetening its inevitable approach and of facilitating the realization of an ancient dream. Other poets have written of that dream.

"When I was a small boy and unhappy," wrote W. Somerset Maugham (1938), "I used to dream night after night that my life at school was all a dream and that I should wake to find myself at home again with my mother. Her death was a wound that fifty years have not entirely healed. I have long ceased to have that dream, but I have never quite lost the sense that my living life was a mirage" (p. 308). Elsewhere (p. 292), he wrote, "There are moments when I have so palpitating an eagerness for death that I could fly to it as to the arms of a lover. . . . It seems to me then to offer the final and absolute freedom." In this unambiguous allusion to reunion in death one may discern the same longing for an eternal embrace that has been cited in the previous pages. What Saint-Exupéry referred to as his "notion of immensity" corresponds, moreover, to passages in Jones's (1911) paper, "On Dying Together," where he remarked that the wish to sleep and die together is the same as the wish to lie together, and that the grave so longed for is simply an equivalent of the mother's bed.

Seen in this light, the claustrophobia against which the appeal of flying seems designed to serve as an antidote may in itself be a defense against a more hidden claustrophilia, an ambivalent longing to return to that "heaven whence we were born." Here flying may be viewed as a special form of travel, that common metaphor for death, from where there unfolds no dichotomy between flying and dying, but a fusion embracing those states, poetically rendered in the concepts of death and resurrection, of angels with wings, and of an ultimate destiny of immortality residing in a distant firmament.

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Kuhn's "Paradigm" and Psychoanalysis

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KUHN'S "PARADIGM" AND PSYCHOANALYSIS

BY VANN SPRUIELL, M.D.

This paper traces the development of T. S. Kuhn's use of the technical term, "paradigm." It questions its loose usage by psychoanalysts. However, Kuhn's developed and more explicitly defined sense of paradigm can be used by psychoanalysis to gain a perspective from which to examine its own disciplinary structure and function.

I

The purpose of this communication is to pose two questions. The first is whether the term paradigm, in its sense as a special, technical term used in the history of science by Thomas S. Kuhn, is used correctly in psychoanalytic writings [1] and therefore does justice to its originator. And if the answer is no, Kuhn is not done justice if his ideas are trivialized (as I believe they often are), then the next question follows: if we survey Kuhn's *developed* and more explicitly defined use of paradigm, does psychoanalysis have anything to gain from its better understanding? I believe that the answer will turn out to be positive, that psychoanalysis can gain from Kuhn's later formulations—at least it can gain a useful perspective from which to examine its own disciplinary structure and function.

II

This is not the place to consider, in any sufficient detail, the discipline of psychoanalysis in the light of the work of T. S. Kuhn. Nor is it the place to attempt to understand Kuhn's

[1] "Paradigm," of course, is a word in the general language, meaning pattern, example, or model; it is also used as a technical word by grammarians.

work in the light of psychoanalytic understandings. Either endeavor would be worthy. *The Structure of Scientific Revolutions*, published in 1962, caused a small revolution in the history of science. It was a revolution in the *history* of science; it was not in itself scientific, but about science. As such, however, it has undoubtedly influenced the various scientific disciplines.

Trained as a physicist, Kuhn became a historian; as part of a natural development, he has become immersed more recently in the philosophy of science. References to his work occur frequently in the psychoanalytic literature, unfortunately with little explication.[2] The purposes seem to be to emphasize Freud's greatness or to point to the coming of his successor. But paradigms and scientific revolutions amount to more than that.

Kuhn concluded early that the conventional textbooks on the history of science were simply wrong, not so much about facts as about processes and sequences. No science primarily develops in steady, small increments—tiny accruals of fact. Science develops in revolutionary spasms, with periods of consolidation between. Both before and after revolutionary changes, any given discipline has overarching theories, some models, favorite metaphors, even systems of symbolization. These ways of thinking—Kuhn called them together *paradigms*—not only define the discipline but can be used to explain most of the phenomena in which the discipline is interested, as did Ptolemaic astronomy or the phlogiston theory.

Most "normal science" is not engaged in radical innovations, lonely and heroic explorations of the unknown. Most normal scientists work with the puzzles for which the contemporary paradigm is applicable. Those puzzles for which the paradigm does *not* apply are typically ignored or even denied entirely. But sometimes these anomalies of explanation cannot be denied,

[2] Too many authors to list have referred to Freud's contribution as a paradigm; Ornstein (1981) believes there have been three paradigms, the first two invented by Freud and the last by Kohut; Eissler (1969) suggests that Freud developed a series of paradigms and exhausted the matter; Rangell (1983) reports that one analyst publicly stated that we have a whole "cafeteria of paradigms"!

either for pressing general reasons (in which case several people are apt to create a new paradigm almost simultaneously) or because some atypical scientist finds the climate right for the acceptance of his ideas. Then a *new* paradigm is created, a new system of thought, which explains more phenomena more parsimoniously and elegantly. Often, Kuhn tells us, there ensues a battle between the conservatives, the adherents to the old paradigm, and devotees of the new ways of thinking. When one side or the other wins, the warriors can return to their puzzle laboratories.

A new paradigm amounts to seeing the theoretical structure of a scientific discipline in some new and useful way. The effect, if innovation takes hold, is revolutionary. If the revolution is a large one, the effector is often dubbed genius, and previous geniuses become denigrated. Revolutions are not, of course, confined to science. For example, only the most obstipant of the critics of psychoanalysis would deny that Freud was both a revolutionary *and* a genius, and so it is understandable that some would be tempted to say that he created a new paradigm. By that is meant that he created a new way of looking at the human mind and that a new body of knowledge grew. But if a revolutionary way of thinking is considered to mean a new paradigm, and new paradigm means by itself revolution, then we have no more than synonyms. Why, then, use the word paradigm in reference to psychoanalysis at all? The answer is easy: paradigms are used by Thomas S. Kuhn in connection with *science*. Psychoanalysts, at least some of them, would like to think of themselves as scientists.

Since the publication of *The Structure of Scientific Revolutions*, Kuhn has discovered to his dismay that all sorts of uses have been made of his concept. In an essay titled "Second Thoughts on Paradigms," published in his 1977 book, he remarks (p. 293) that "part of the reason for its success is, I regretfully conclude, that it can be too nearly all things to all people." He blames himself for the excessive plasticity of the term, even repeating one critic's observation that there are at

least twenty-two different usages of "paradigm" in the first book. Kuhn was particularly unhappy to find the term invoked by social and behavioral scientists—and psychoanalysts. He had intended it to refer only to the so-called hard sciences, for only they have the sorts of puzzles which can be solved by adhering to certain shared rules within a discipline. For Kuhn, it is the finding of solutions to particular puzzles which defines what is specific about scientific work.[3]

Kuhn (1977, pp. 280-282) rejects Popper's (1959) invocation of falsifiability as the primary criterion for any proposition to be considered truly scientific. Kuhn's criterion is the shared capacity of a community of professionals to solve certain kinds of puzzles. Popper argues that the tradition of critical discussion—claims, counterclaims, and debates over fundamentals—is crucial for science and began with pre-Socratic philosophers. But Kuhn retorts that these activities, which have also characterized philosophy and much of social science ever since, are not the essential characteristics of science. In fact, by the Hellenic period, mathematics, astronomy, statics, and the geometric aspects of optics had abandoned this mode of discourse in favor of puzzle solving. "Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy" (1977, p. 273).

[3] In a paper decrying the lack of scientific research within psychoanalysis, Holzman (1976) urges the movement of psychoanalytic institutes into general universities, partly because he believes that "Most scientific revolutions emerge from within the academic structure . . ." (p. 263). He takes the position that psychoanalysis is in *itself* a "paradigm," that is, it has to do with the "characteristics of nonconscious thinking" (p. 258). He points out that the three classes of events, which, according to Kuhn's original work, follow the appearance of a scientific paradigm, generally fail to occur with any regularity or vigor within pyschoanalytic institutes. These are: the investigation of the facts intrinsic to the paradigm, specific predictions that can be made by its use, and attempts to resolve ambiguities in its formulation. However valid his arguments, he avoids Kuhn's point that ordinary scientists typically neglect those areas in which the paradigm does not apply or is ambiguous. And his use of "paradigm" is simultaneously more broad and more narrow than what Kuhn had in mind. The core of psychoanalysis cannot be reduced to nonconscious thinking alone; it is made up of a set of basic concepts of varying degrees of verifiability.

To attempt to reduce the confusion, Kuhn wishes to replace paradigm in the broadest sense with the term, *disciplinary matrix*, " 'disciplinary' because it is the common possession of the practitioners of a professional discipline and 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification" (1977, p. 297). A disciplinary matrix has various constituents, including *symbolic generalizations* (formal or readily formalizable components), *models* (e.g., a gas behaves like a huge collection of tiny, randomly moving billiard balls; electrical circuits can be usefully seen as if they operated like hydrodynamic systems), and *exemplars*. Exemplars are concrete problems, which can be solved by the forms provided. Kuhn would rather the word paradigm refer to this narrower part of the disciplinary matrix, that of exemplars. Exemplars are like the problems found at the ends of chapters in scientific textbooks. They are puzzles.

"Puzzles are, in the entirely standard meaning here employed, that special category of problem that can serve to test the ingenuity or skill in solution" (1977, p. 36). Thus, the interest is finding a solution to a problem, along pathways shared by the community, for which there *is* a solution. Many social or social science problems are not puzzles in this sense at all. Nor is utility the important issue. What then challenges the "normal scientist" is the "conviction that, if only he is skillful enough, he will succeed in solving a puzzle that no one before has solved or solved as well" (1977, p. 38).

III

Freud wished, for whatever reasons, to identify psychoanalysis with science. But he recognized, of course, that "*our* science" is unique in that both subject and object of the investigation are similar: easily deceivable minds (1940, p. 159). Gabriel (1982), in an excellent paper on the influence of psychoanalysis on other sciences, traced the nature of Freud's commitment to positivism, the ways in which Freud's thought broke out of the

constraints of positivistic thought, the aridity of the debates with philosophers of science about whether psychoanalysis is or is not a science, and the need for psychoanalysis to identify itself with *itself*, to examine itself in its own terms. The search for similarities can hide what is unique. The wish to be acceptable to philosophers like Wittgenstein (in the face of whose critical analyses not even the "hardest" science can hold its footing), or to try to measure up to the criteria of positivist philosophers, like Hook or Popper, can be debilitating if not destructive.

The thirst for the prestige of science or the approbation of philosophers to one side, words like paradigm and phrases like "scientific revolutions" may come to be degraded in meaning. They may come to be used merely as pretentious clichés. However, we might find Kuhn's *latest* way of putting things useful if we look for true analogies in our own field, that is, if we think metaphorically.

IV

One might conclude that psychoanalysts do not have paradigms even in the narrow sense, unless work within a single analytic situation might be considered such. But is that all there is? Is it true that, because certain problems cannot be easily stated in a textbook or symbolized on a blackboard, we do not have something analogous to paradigms as exemplars? One possible, though partial, answer will be discussed below.

In any event, Kuhn does not consider psychoanalysis a science because it has no puzzles in the specific sense of paradigm as he means it. This is not to say that he does not regard psychoanalysis a worthwhile activity or that he denies that it can ever become a science. Recently, in an informal discussion with a small group of psychoanalysts,[4] he acknowledged that there might be activities of analysts not readily accessible to out-

[4] At a meeting of the Center for Advanced Psychoanalytic Studies, Group VII, on March 12th, 1978, in Princeton, N.J.

siders, like himself, which in fact do represent work comparable to that done with paradigmatic exemplars. But, the questions of whether psychoanalysis does or does not already have some activities at least analogous to exemplars, or whether it approaches or ever will approach scientific status, remain problematic.

At present, our discipline cannot demonstrate to Kuhn and other independent observers the crucial characteristics defining what they mean by a "science." In itself, there is nothing bad about that. Kuhn would certainly claim scientific status for his original field, physics, but equally certainly would not claim it for his current fields, history and philosophy. Yet he obviously regards history and philosophy as worthwhile activities, developing their own truths. None of us really believe scientific truths are all we can count on. Otherwise, we could not feel reasonably sure that Abraham Lincoln was President during the Civil War because that fact could not be surely demonstrated scientifically.

At any rate, psychoanalysis does *resemble* the scientific disciplines described by Kuhn, at least in some respects. Like them, a disciplinary community grew up, held together in various ways. This community shared a small number of preferred metaphors or models for its structure and operations. It also engaged in efforts to apply similar techniques in its activities. In this sense, we do have, I believe, a disciplinary matrix—but one not easy to define. In one sense, there is some *danger* in attempting to define our basic concepts too carefully. Nevertheless, most psychoanalysts would agree that they follow certain craft rules—for example, in utilizing free association or in placing specific limitations on the analyst's activities. Freudian analysts agree about what they mean by psychic determination, the importance of the dynamic unconscious, the principles of pleasure and multiple function, the principle of the complementary series, a drive theory (or something equivalent), a concept of drive regulation (or something equivalent), an epigenetic developmental theory, notions of object relations,

internalization, the acquisition and alteration of psychic structure, and derivative concepts of defense, resistance, transference, and the like. Crucial in all these basic concepts is that of ubiquitous intrapsychic conflict.

Perhaps most of all, we establish the psychoanalytic disciplinary matrix—as Kuhn points out all sciences do (1977, p. 237)—in a highly defined social situation, the *school*. The psychoanalytic institute provides a setting for both candidates and teachers to establish or continually renew the basic models and precepts which make up the disciplinary matrix. The professional and scientific societies, in part, serve the same function: to preserve the community. Institutes, like schools devoted to sciences such as physics or chemistry, do not encourage early innovations by students; truly useful innovations are most likely to come after a solid position within the discipline has been achieved (Kuhn, 1977, p. 239).

As for the constituents of the disciplinary matrix which Kuhn mentions, it would be fair to say that the attempts to develop highly abstract symbolic generalizations have so far been primitive in comparison to the hard sciences but sophisticated in comparison to most of the behavioral sciences. We do not lack symbolic generalizations, but they remain on the level of verbal entailment propositions (see Rosen, 1974). Of models, we have a variety, perhaps the most useful of which is the organismic—in ways like the organismic model of general systems theory. But what about exemplars—the *narrower* meaning of paradigm? Do we have puzzles to work with the aid of shared rules, puzzles which are at least analogous to Kuhn's puzzles?

We do have at least an analogy. It is the small case conference, usually made up of peers, who examine some piece of clinical material or the work with an entire case. Here, something astonishing happens—at least it continues to astonish me. When a good case conference takes place, the analysts as a group draw closely together, work well together, usually with productive disagreements and great interest. The group disassembles the

elements of the material and reassembles the parts in various ways. More often than not, the group is able to arrive at a consensus in its formulations.

Although these activities of ours are almost impossible to demonstrate in public discussions, unlike our colleagues from other disciplines we ourselves have participated in psychoanalytic case conferences, perhaps discussing a specific dream, the patient's associations, the analyst's thoughts, feelings, and interventions. With great regularity, the participants in such conferences, if they are professionally competent, are able to *understand* the process, *discriminate* interferences when they are there, *reliably infer* the deep structure of unconscious operations from the presenting material, and make successful *predictions* (it over and over turns out) referring to the course of the analysis (if all other elements of the person's life and experience hold steady). In fact, the group operations of experienced psychoanalysts represent perhaps the most fertile field for future scientific investigations.

Whether the psychoanalytic disciplinary matrix needs to be altered much at present, or whether any new matrix is even remotely in view, can only be tested slowly and gradually in the privacy and intimacy of the consultation rooms and in the semi-privacy of serious clinical discussions in small groups. This has been the case at least for sixty-five years. Jungs, Stekels, Adlers, Ranks, Ferenczis, Horneys, Fromms, Sullivans, and Rados have come and gone, each, however, adding something to the corpus of psychoanalytic knowledge. But not one of them so far has had his way with its core. We are continually being confronted by proposed equivalents of new paradigms, whether seen in the broader sense more properly termed disciplinary matrix, or in the narrower sense of exemplars. So far, none has succeeded the final tests of time and experience—the marketplace, if you will. Some day, no doubt, one will come. I venture to guess that when it comes it will be a surprise. We will have little to do with either consciously planning it ahead, or, after it comes, individually influencing it one way or another.

V

Kuhn's work has been enormously influential. That an immersion in two diverse fields such as physics and history can have such productive consequences is documented by his own work. That it produces oddities is also revealed by Kuhn's statement (1977, p. xv) that he was influenced in *history* by models conceived in the post-Kantian tradition; but as a *physicist* or (presumably analytic) *philosopher*, he continues to find these models opaque. Ironically, Kuhn, like it or not, is caught in the same dilemmas in which the psychoanalyst finds himself. Insofar as the psychoanalyst would like to think of his field as at least a protoscience, perhaps like economics, he tends to think in philosophical terms (whether he knows it or not) either like those of the nineteenth century positivists or like those of the English analytic philosophers. Insofar as he thinks of psychoanalysis as a discipline analogous to history, cultural anthropology, or linguistics, he thinks like some of the modern neo-Kantians, or like the French or Chomskian structuralists, or like general systems theorists. Straddling ways of thinking, Kuhn and psychoanalysts have much in common. Perhaps this is the point: psychoanalysis should struggle with what it *has*, rather than pine after either only scientific "legitimacy" or only hermeneutic ventures into the interpretation of "meaning." Such is the way not of eclecticism but of the endurance of ambiguity.

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THE CURRENT STATUS OF THE PSYCHOANALYTIC THEORY OF INSTINCTUAL DRIVES

I: DRIVE CONCEPT, CLASSIFICATION, AND DEVELOPMENT

BY ALLAN COMPTON, M.D.

This paper proceeds from the basis of a scrutiny of the evolution of Freud's drive theory to survey and synthesize the major trends in and problems of recent and current contributions to the psychoanalytic theory of the concept, classification, and development of instinctual drives.

The relationships between body, mind, and environment present a central and unavoidable problem for any serious psychology. Historically, in psychoanalysis the theory of instinctual drives has been the primary conceptual device for dealing with these relationships. More recently, the propositions of ego psychology have added a complementary set of concepts which also deal with mind-body-environment interactions.

Today the concept of instinctual drives continues to be one of the most pervasive concepts of psychoanalytic literature, even in the writings of those authors who are highly critical of drive theory. Virtually every psychoanalytic paper utilizes or has bearing upon some aspect of the theory of instinctual drives. Despite this pervasiveness—or perhaps because of it—there have been very few attempts to scrutinize drive theory as a whole, to review its development, to summarize changes, or even to piece out the relationship of the elements of drive theory to one another. The standard reference remains Bibring's (1936) review of Freud's work (see also, Arlow, 1959; Brenner, 1955, 1982; Fenichel, 1945; Nunberg, 1956; Waelder, 1960).

The purpose of this study is to trace the development of the psychoanalytic theory of instinctual drives in the last forty years. The formulation that there are three phases of psychoanalytic drive-structure theory that are incompatible in certain respects (Compton, 1981a, 1981b, 1981c, 1981d) will be used to understand these post-Freud contributions. To what degree has our concept of drives changed? Have any of the problems inherent in Freud's drive theory been solved or reduced? Have any been magnified or enhanced? What difficulties in or advantages of psychoanalytic drive theory have emerged?

A comprehensive survey of the recent literature on instinctual drives is not intended. I shall instead be concerned, in this paper and in the companion paper that immediately follows it, with those contributions which have particularly helped to focus the ongoing issues of drive theory. In this paper my discussion will be organized in the following sequence: (1) the concept instinctual drive; (2) the classification of instinctual drives, including the theory of aggression; and (3) the development of drives.

I: THE CONCEPT INSTINCTUAL DRIVE

In Freud's most extended definition of instinctual drive (1905 [1915], p. 168), he said that a drive is the "psychical representative" of a force, pressure, or demand that arises in the body as excitation in an organ. Its aim is the removal of excitation in that organ. Instinctual drive is a frontier concept, to be understood as a demand for work made by the body upon the mind and in itself is without quality. This concept of instinctual drive, the one with which psychoanalysis grew up, clearly emphasizes the relationship of mind and body as the central issue.

Freud also proposed (1915a, pp. 122-123) that any instinctual drive might be characterized according to its source, impetus, aim, and object, adding (p. 123) that exact knowledge of the source is not necessary for psychological investigation and is, in fact, outside the scope of psychology. In this statement Freud

explicitly recognized a problem that arises for psychoanalysis: observational data are primarily psychological; a core concept has a central somatic referent; there is built-in ambiguity in mind-body terms.

Most of the basic issues of the concept of instinctual drives can be approached on the basis of these statements by Freud. In listing these issues, I shall necessarily include most of the fundamental problems of human psychology. That necessity simply reflects the nature of the psychoanalytic concept of drives.

(1) What is the relationship between drive as a concept and the data of observation?

(2) In those areas which involve "interactions" between body, mind, and environment, how are the conceptual gaps and discontinuities to be dealt with? Does the nature of the observational data of psychoanalysis necessitate doing without somatic referents in the formulation of theoretical constructs? What is the effect of formulating constructs in a "purely psychological" way?

(3) How is the ontogenetic origin of mind to be accounted for in the context of drive theory?

(4) Is it possible to state meaningful hypotheses which include both force terms and meaning terms?

(5) What is the relation of the psychoanalytic concept of instinctual drives to the biological concept of instinct?

(6) If one accepts the construct of mental systems, thus aligning drives with one of the systems, drives must then be viewed theoretically in terms of interactions between the systems. Do we do this successfully? Is it of value?

Understanding the first issue is a prerequisite for the rest. *What is the relation of the concept of drive to observational data?* For the moment I shall use Freud's pre-1920 formulations as a context, and sexuality as the sample drive, returning later to modifications necessitated by the ego-id-superego model and aggression. The question may be approached clinically or theoretically; the reasoning is different.

Clinical approach. As Freud pointed out (1915c), the primary data of psychology are the elements of conscious awareness: thoughts (ideas), feelings, perceptions (including sensations), and a sense of more or less—more or less clarity, vividness, value, strength, urgency, intensity, and so on. The method of psychoanalysis is not, however, introspection. Psychoanalytic method depends upon the inference that other people have a consciousness similar to one's own. Given that inference and the psychoanalytic method of observation of (reports of) mental process (conscious content), gaps in conscious awareness are abundantly evident. We then make a second inference: there must be unconscious mental processes; we do not and cannot know them, but say that they consist of elements such that, if they were conscious, would be thoughts, feelings, and a sense of more or less. Using the second inference, we "discover" certain kinds of processes and contents to be characteristic of the unconscious part of the mind—urges (or impulses or wishes or fantasies), tending to be sexual or aggressive in their inferred content and traceable to childhood. We now ask: what gives these elements their apparent impetus? And we make a third inference, that the impetus stems somehow from particular somatic connectedness or somatic sources. Thus, the concept of instinctual drives is inferred from clinical data in three distinct steps. Certain elements of behavior, including physical behavior and conscious awareness, are then said to be partially explained as results of derivatives of drives. Instinctual drive is thus an explanatory construct and one which depends upon the inference of unconscious mental processes. An instinctual drive is never seen as behavior, nor is a drive ever manifest as such in consciousness. The data of conscious awareness are thoughts, feelings, perceptions, and a sense of more or less, not drives.

Theoretical approach. An instinctual drive, as defined by Freud, may be said to occur as a result of somatic demands for work in the form of unconscious wishful impulses seeking satisfaction. Derivatives of these wishful impulses—drive derivatives—may be involved in the formation of unconscious fanta-

sies and, in the form of further derivatives, may become conscious as ideas, feelings, and a sense of more or less. From this approach the primary data of consciousness may be seen as dependent variables, partially determined by drive derivatives and, eventually, by drives. Drives are seen as, approximately, independent variables within the mental systems. Again there is no implication that drives themselves are ever directly observable. Statements about drives are not statements about physical or mental behavior. Instinctual drive is an explanatory construct.

Does the nature of the observational data of psychoanalysis, and the relationship of that data to theoretical constructs, necessitate doing without somatic referents in the formulation of theoretical constructs such as that of instinctual drives? Must we create a "purely psychological" concept of drive?

In recent years there has been a distinct trend toward purely psychological concepts of drive. Brenner (1955), for example, said that a drive is

> a genically determined, psychic constituent which, when operative, produces a state of psychic excitation or . . . tension. This excitation or tension impels the individual to activity. . . . This activity should lead to something which we can call either a cessation of excitation or tension, or gratification (p. 17).
>
> [Drive] does not include the motor response but only the state of central excitation in response to stimulation (p. 16).

While this definition does not exclude the mind-body relationship, that element is far less prominent than in Freud's definitions. Brenner, in more recent work (1982), specifically eschewed the "frontier" aspect of Freud's definition (see below).

Loewald (1971), who preferred the term "instinct" and discussed instincts in the context of the concept of motivation, was very explicitly purely psychological:

> . . . I consider instinct a psychoanalytic psychological concept, which should be kept free of biological and ethological connotations. . . (p. 105).

> [Instinct] in psychoanalysis . . . is a mental force or stimulus . . . the most primitive element . . . of motivation . . . . a motivational stimulus, constituent of the stream of mental life, and not a biological stimulus operating upon that stream (p. 109).

Loewald chose to emphasize Freud's phrasing that "instinct" is the "psychical representative" of a physiologic stimulus: what the organic stimuli stimulate is the "faculty of representing, an activity which is then seen as inherent in the mind. . ." (pp. 107-108).

Loewald has, in his effort to be purely psychological, run aground on one of the basic issues of drives, the *problem of the ontogenetic origin of mind.* This problem may be profitably viewed longitudinally or cross-sectionally. In the longitudinal sense, the human organism develops from the fertilized egg to the physically and psychologically mature adult in an infinite number of steps that can be variously organized. But how or when does mind enter into this developmental sequence? In the cross-sectional sense, at any given moment, after some particular developmental point, we see somatic processes as somehow or other reflected in certain aspects of what we study as mental processes. As Brenner said (e.g., 1982), mind is a function of brain. But how, at any given moment, does this occur?

These questions are not usually made explicit in the psychoanalytic literature, but the positions tacitly taken which serve to answer them are the apparent source of many nonproductive disputes. The terms of these positions tend to be treated as "fundamental indefinables" of our science, which they are not. Some examples are: Freud's phylogenetic precipitates; perhaps Freud's concept of psychic energy; apparently inherited unconscious fantasies (Jung, M. Klein), which are present at birth and form the "deepest layer of the mind"; the strange word "mentalization" which has been appearing in recent literature, as "internalization" becomes more recognized as problematic; "internalized object relations" (Kernberg, 1976, e.g.). Loewald's "faculty of representing" is another such position: it does not

improve Freud's formulations about drives, but only buries the question of the ontogenetic origin of mind. Mind is, in Loewald's formulation, apparently synonymous with the "faculty of representing," or the faculty of representing is how anything, including a drive, can become "mentalized." Rather than the drive coming to the mind and demanding work, the faculty goes to the drive and creates representation. Ambiguity and discontinuity exist in both formulations. In this circumstance, explicit ambiguity, as in Freud, seems more propitious.

Rapaport (1960) seems to have introduced to the psychoanalytic literature the attempt to place drives within the context of "motivation" as a more general concept. He proposed that all behavior is causally determined, but not all behavior is motivated; that is, motives should be distinguished from other causes. Other causes may be classified as physical (mechanical), somatic, and social (pp. 862-863). He defined motivation as an appetitive force, not equated with any specifiable physiologic process, experienced by the subject either as actively willed or passively suffered. Appetitiveness, the key word, is characterized by pre-emptoriness, a cyclic pattern (leading to consummatory activity), selectiveness, and displaceability (p. 865). Selectiveness, he said, implies "that the direction of the motive force is determined by its object . . . . there are objects which are the specific necessary conditions for the discharge of the energies of the motivations in question" (p. 865). These definitions are apparently intended to allow for psychic forces which are nonmotivational (as well as to provide a set of purely psychological terms for defining drive). Defenses, for example, are not appetitive, have no object, and are not motives (p. 866).

Rapaport acknowledged that what he had done was to give instinctual drives another name—motivations (pp. 866-867). Apparently, he wanted to avoid foreclosing the possibility of nondrive motivations, as well as to allow for nonmotivated determinants of behavior, that is, causes. He then went on in an attempt to improve the concepts of Freud's drive theory, unfortunately also attempting to ascribe the improvements to Freud.

Rapaport was trying to show that Freud's drive theory is purely psychological (pp. 879-880), which it clearly is not at any of the steps in its evolution, saying that the attempt to discuss sources leads to "the impression of a primitive peripheralist theory of instinctual drives" (p. 880). If somatic sources are not to be discussed in the theory, how does one deal with, for example, the sequence of somatic changes in sexual excitement and release? Rapaport said, "The direct manifestations of an instinctual drive are the observable consummatory actions. The other effects of instinctual drives are termed instinctual drive representations (or presentations)" (p. 881).

In the last statement Rapaport seems to be in trouble with both the relation of the drive concept to observational data and the mind-body conceptual gap. Among the causes of (physical and mental) behavior are motivations, defined as appetitive forces not equated with any specifiable physiologic process. Among the appetitive forces—in fact, the only specimen of them—are instinctual drives; Rapaport merely exchanged words. Instinctual drives have mental effects, which are termed "drive representations," and other effects, termed "direct manifestations," which are the "observable consummatory actions"—that is, physiologic processes. Rapaport, in effect, reintroduced and buried the ambiguity he was trying to eliminate and added a statement about direct manifestations of drives which is at odds with the conceptualization of Freud and most other analysts.

More recently Stoller (1979) has also opted for a purely psychological theory, in this case of sexuality or, rather, sexual excitement. He elected in his discussion of the generation of sexual excitement, to "put aside" special physiologic factors and direct stimulation of body parts (pp. 6-7). Later he said, "We shall never make it work with a pseudoanalysis (metaphysics) of psychic energy; rather, psychoanalysis is the study of, the search for, meaning" (p. 193). Stoller gave an extended and valuable report of the role of sexual fantasies—"scripts"—in the erotic life of his patient, attempting to reduce the role of "metaphoric usages" in psychoanalytic theory (referring chiefly

to psychic energy). At one point, however, he said, "It is the nature of mucous membranes to be charged with potential for that form of pleasure we label erotic. The quality of the sensations is ultimately of physiologic origin and so is hard to deny" (p. 107). Stoller would seem to be back in the same dilemma: mind-body ambiguity masked by (a new) metaphor, "charged with potential." In this respect Stoller's work typifies that of many others who strive to take atheoretical approaches: unacknowledged theoretical assumptions creep into formulations masquerading as pure description or metaphor, and scrutiny reveals the formulation to have fallen upon one or another of the basic issues of drive theory.

G. S. Klein (1976a, 1976b), Gill in his more recent work (for example, 1976), and Holt (1967, 1976), among others, also tried to work out purely psychological theories of sexuality or of drives generally, and to divorce psychoanalytic theory entirely from concepts of mental energies.

Brenner (1982) has recently developed his ideas further, in a direction which attempts to obviate some of these difficulties. He took the position that the weight of psychoanalytic—that is, clinical, psychological—evidence is strongly in favor of libido and aggression as driving forces in mental life, but that the same cannot be said of the mind-body frontier concept of drives.

> All of psychology is an aspect of the functioning of the central nervous system. There is no frontier between mind and body . . . mind is an aspect of cerebral functioning. . . . Since all mental functioning is somatic, it is unnecessary and, in some respects, even misleading to separate one group of psychological phenomena from others by labeling them as somatic in their nature (pp. 19-20).

What, then, of the relationship between libido and the erotogenic zones?

> It is important to emphasize the following with respect to libido, however. Libido is closely tied to and influenced by what Freud called the erogenous zones as well as by various sexual metabolites. . . . Nonetheless, it is worth repeating that there

> is no convincing evidence that the erogenous zones are, in fact, the sources of the libidinal drive. To be sure, wishes for satisfaction of one sort or another are profoundly influenced by many factors, among which stimulation of the erogenous zones figures largely. . . . However, such stimulation, hormones, etc., are not sources of libido. . . . Libido, like all other psychic phenomena, including aggression, derives from the functioning of the brain. It is one of the features of the aspect of brain functioning we call the mind (p. 24).

Where does this leave us? Certainly, the peculiar notion that some special excitation is somehow transmitted from the erotogenic zones to the mind, to which I have repeatedly objected (Compton, 1981a, 1981b, 1981c, 1981d), is eliminated. Perhaps it is satisfactory to allow it to stand, for the time being, that the problems of the relation of erotogenic zones to mental forces are problems for neurophysiology and for the study of neurohormonal interactions—or at least that it is impossible to say more. Brenner has, in any event, stopped short of inventing a phrase which has the effect of obscuring a basic problem. He has also remained within the confines of his own rigorous definition of drive as the producer of a state of psychic excitation. He provided a clear somatic referent in saying that mind is an aspect of cerebral functioning.

We can, however, raise the same question here. Does the nature of psychoanalytic data necessitate purely psychological formulations of the construct instinctual drive? It would seem to be the essence of Freud's drive concept that certain physiologic processes which commence, or may commence, outside of the central nervous system have a special relation to psychological phenomena, mediated, of course, through structures and pathways of the central nervous system. And, at least where sexuality is concerned, it is this special relation that is intended to account for the driven-ness. Something essential would seem to be lost by eliminating this relationship as an integral factor in the explanatory construct of drives.

Thus far, I have been discussing contributions affecting the

psychoanalytic concept of instinctual drives in the framework of the first three basic issues I listed—concept/data relationship, body/mind/environment relationship, and the problem of the ontogenetic origin of mind. All of the basic issues may be seen as facets of one comprehensive problem, and Ricoeur (1970), although he was not focusing on the concept of instinctual drive and approached issues from a very different viewpoint, grasped, I believe, very clearly, another of the facets when he said,

> What is the status of representation or ideas in relation to the notions of instinct, aim of instinct, and affect? . . . How can the economic explanation be *involved* in an interpretation dealing with meanings; and conversely, how can interpretation be an *aspect* of the economic explanation? It is easier to fall back on a disjunction: either an explanation in terms of energy, or an understanding in terms of phenomenology. It must be recognized, however, that Freudianism exists only on the basis of its refusal of that disjunction (p. 66).

He made a similar point in 1977: "in short, practice forces us to think of meaning and force together in a comprehensive theory" (p. 850; see also, p. 856). Freud's insistence on propositions that are ambiguous in mind-body terms represents this same recognition of the necessity for a conjunction of meaning and force.

We have noted thus far two factors in the search for purely psychological concepts of instinctual drives: respect for the nature of observational data and the advantages of circumventing the mind-body problem. Ricoeur's observations bring into focus a third factor: dissatisfaction with the concept of psychic energy is connected with dissatisfaction with the somatic aspects of the concept of drive because energy was one of the main ideas in which Freud housed the mind-body ambiguity. It was also Freud's device for "getting the mind started," and, in Ricoeur's terms, refusing the force-meaning disjunction. While there is much to be said concerning the disadvantages of the concept of psychic energy and how we use it, I agree

with Brenner (1980) in this respect:

> . . . in that theory there should be some term to designate the concept that drives have a capacity to impel the mind to activity —a capacity that varies in strength from time to time. What that concept is *called* matters not at all. . . . Just changing the *name* from "drive" to, say, "motivation" changes nothing in the theory. It makes the theory no more "psychological," no less "mechanistic," no more "human," than it was before (p. 211).

It might be added that, on empirical grounds, there must be some conceptual device to account for the sense of more or less.

A related issue has appeared as a terminological problem: whether to translate "*Trieb*" as "instinct" or as "instinctual drive." The issue, of course, is the relation of the psychoanalytic concept of instinctual drives to the biologic concept of animal instincts. Hartmann (1948), preferring "instinctual drive" to emphasize the difference between the two concepts, cited the following characteristics which differentiate human drive-related behavior from animal instinctive behavior: relative independence from outer stimuli, leading to greater plasticity of adaptive behavior; greater variability of responses to inner stimuli; continuity of driving forces (pp. 78-79). Brenner, too, clearly differentiated instinct from drive, emphasizing the state of central excitation as the drive, with the quality of plasticity ascribed to mediation of motor response by ego functions (1955, p. 16; see also, Waelder, 1960, pp. 97-102).

The biological concept of animal instincts has also changed (see Fletcher [1957] for a survey of usages, and Wilson [1975] for an approach to the same phenomena essentially minus the instinct concept), and presently bears little resemblance to Freud's tension reduction/reflex arc model of 1900. Kaufman (1960), for example, made a very convincing case for the idea that instinctive behaviors in animals are interrupted by certain stimuli—*terminating stimuli*—rather than by a discharge of specific energy. An example is a bird that normally lays four eggs but continues egg-laying if eggs are continually removed from the nest so that less than four are present.

Such studies raise significant issues for psychoanalytic drive theory, even though the concepts are certainly not directly transposable. If a carefully phrased definition such as Brenner's, in terms of "psychic excitation," is accepted, what would be the effects of using a cybernetic model, rather than a reflex arc/discharge model, as a basis for the drive concept? I have suggested elsewhere (Compton, 1981a) that Freud's early "Schematic Picture of Sexuality" (1895a) had a more prominent feedback loop quality than his later drive theory. Hartmann (1948, p. 72) has pointed out that, historically, the study of aims and objects became far more prominent in psychoanalysis than the study of sources. A cybernetic model of drives might afford a more balanced recognition in the theory for this historical trend of increasing psychological emphasis, and also permit further de-emphasis of drive-specific energies while not necessitating a detachment of psychological from somatic functions. Other problems are, however, readily introduced.

Considering the concept of instinctual drives leads into a number of issues in addition to those formulated above as questions. Many of those additional issues and the question concerning the structural position of drives will be discussed in the sections following. It is not my intention here, however, to try to resolve that list of weighty, fundamental problems which the drive concept of psychoanalysis brings into some degree of focus—or even to resolve the less formidable issues to be noted subsequently. The work of Brenner, Hartmann, Loewald, Rapaport, Ricoeur, and others has served to clarify some of the problems in Freud's drive theory, whether or not we find their proposed solutions entirely acceptable. The recurrent idea that a "purely psychological" drive concept can obviate some of these difficulties is tempting on methodologic grounds, but it creates other problems and seems to do violence to Freud's fundamental drive concept.

Various axiomatic positions have been taken, historically and currently, which foreclose most of the fundamental issues cited here. Such positions do not resolve anything and lead to unpro-

ductive theoretical disputes. Solutions to the fundamental issues which drive theory brings to light must be approached with skepticism. There is, however, no reason not to make use of the current, more accurate, and sophisticated models in neurophysiology and ethology, and no reason not to utilize new concepts derived from cybernetic and information theory, or even sociobiology, to alter and improve psychoanalytic drive constructs, and to strive for greater homology with data from other avenues of approach to behavior. Such efforts must be differentiated from translations of old concepts into new terms which "sound better" but have the effect of burying the fundamental issues of human psychology which Freud's drive theory exposes.

II: THE CLASSIFICATION OF INSTINCTUAL DRIVES

In the 1920's Freud abandoned his earlier classification, sexual drives and ego drives, and, at first tentatively, then with increasing conviction, postulated life and death drives as manifestations of omnicellular, biological forces. He never accepted aggression except as a possible derivative of the death drive, as Brenner has pointed out (e.g., 1971b). Freud maintained the derivative nature of aggression, at least in part, because of the lack of an evident somatic source for aggression and the lack of a clear relationship to the pleasure principle.

Freud's postulation of life and death drives, the latter based on the idea of a repetition compulsion, caused considerable debate and controversy in the 1930's and 1940's (see Loewenstein [1940] for a carefully considered discussion). Although Freud's life and death drives were never generally accepted by analysts, no sound theoretical statement upon which to base an independent aggressive drive existed until 1949. Hartmann, Kris, and Loewenstein then worked out the necessary assumptions and their consequences. They were willing to forgo a discernible *source* for aggression, basing the concept on psycho-

analytic clinical (psychological) data. They assumed that aggressive discharge of itself may be experienced as pleasurable, that is, be regulated by the pleasure principle, and that, with respect to *impetus*, libido and aggression might be held to operate in strictly parallel fashion. Libidinal *aims* were seen as varied and plastic; the aims of aggression were taken to be more rigid, but the concept of the aims for aggression was recognized as far less clear. They proposed that both the aim and the *object* characteristics of aggression require a somewhat different discussion than do the corresponding aspects of libido, discussion in terms of modification of aims and modification of impact upon the object. Aggressive trends, they proposed, may be sublimated or aim-inhibited, as are sexual trends. Developmentally, aggression was seen to follow the patterns of libidinal discharge modes—that is, the sequence of libidinal organizations postulated by Freud might be taken as (libidinal/aggressive) drive organizations.

Despite the widespread influence of the paper by Hartmann, Kris, and Loewenstein, most issues regarding aggression as an instinctual drive seem to remain unresolved, and analysts remain widely divided on the issues of aggression. Stein (Panel, 1972) suggested that four groups may be seen: (1) those who follow Freud's formulation of the death instinct (Eissler, 1971; M. Klein, 1958; Rosenfeld, 1971); (2) those who consider that aggression and sexuality may be viewed in a parallel way, including an underlying aggressive energy comparable to libido (Brenner, 1971a; Hartmann, Kris, and Loewenstein, 1949); (3) those who reject the concept of aggression as an instinctual drive, seeing aggression as a result of frustration and nonspecific tension reduction (Fenichel, 1945; Gillespie, 1971; Stone, 1971); (4) those who would prefer to dispense with drive models entirely.

The issues here are obviously broader and more complex than those of the concept of aggression itself. Our concept of instinctual drives determines what phenomena we will try to explain by drive constructs. A problem of drive constructs is

that they can readily explain too much: a different drive can be—and has been—postulated as an explanation for every conceivable type of behavior. Circumscription of the number of drives then becomes essential if the concept is to be useful or even meaningful. Such circumscription is achieved, as in other sciences, by classifying behavior according to features which are then explained by corresponding classes of drives. The classes of drives selected determine the universe of discourse for the instinctual drive explanations. Questions may then be raised concerning the concept, the method or basis of classification, or the content of the classificatory scheme. Scrutiny of any of these areas may lead to a conclusion that things we have called the same are not the same after all, or that they are the same, but not what we have called them.

Such discussion tends to be determined by the content of the current classificatory scheme, and the historical dimension becomes important. Historically, in psychoanalysis there has been a sequence of drive classifications, as noted above, always consisting of two groups. When the universe of discourse contains only two classes, questions about one of the members are inevitably questions also about the scheme of classification and the drive concept itself.

The question of how we decide how many drives there are resolves into two other questions. *How do we decide whether or not a given phenomenon is to be partially explained by a drive construct?* And, *how do we decide whether a given phenomenon, or its explanation, is to be subsumed under a particular drive construct rather than another one?*

There have been a number of suggestions for other classifications of drives or for adding instincts in the biological sense, mostly in relation to the explanation of anxiety. Brunswick (1954), for example, proposed the addition of defensive drives to libidinal and aggressive, and a reclassification into vital-libidinal and defensive-aggressive types. Schur in 1958 proposed an instinct (not drive) to avoid danger, present in animals and men, an idea which he later (1966) extended. He saw the bio-

logical withdrawal response as the model for the anxiety response and for defense (pp. 151-152). In Schur's formulation also the instinctual drives, libidinal and aggressive, are regulated by the pleasure principle, while the instinct to avoid danger is not. The unpleasure principle is related to the avoidance of pain or danger as an instinctive reaction in itself. Rangell (1955, pp. 403-404; 1968, pp. 398-399) and Stewart (1967) also suggested instinctive factors in the production of anxiety. So, too, did Freud, in the idea of "key neurones," which appeared in the *Project* (1895b) but never afterward. These elements of the nervous system were proposed to generate unpleasure, that is, were a source of negative impetus parallel to what he later called libido in the sphere of sexuality and pleasure.

These, or other, alternative views of drives or instincts may have more merit than their lack of acceptance to date would indicate.

Within the framework of the libidinal/aggressive classification, most of the current problems have to do with aggression. The following issues arise concerning the concept of aggression as an instinctual drive. What are the observational data upon which we base the concept of an aggressive drive? What is the relationship between these data and the explanatory construct? To what degree and in what respects is the concept of aggression as an instinctual drive different from or parallel to the concept of libidinal drives? The issue of parallelism to libido raises questions concerning the applicability of the characterizing aspects of source, impetus, aim, and object to the concept of an aggressive drive seen as an explanatory construct. Alternatively, we might see the issue as one of altering the concept of instinctual drives to accommodate aggression as we understand it.

For the psychoanalytic drive model, questions of source and relevance of observational data are closely linked. For the discussion of aggression and source, the earlier statement of this relationship requires expansion.

There are various bodily events which may come to awareness more or less readily, via the activity of afferent (sensory) nerves and their central nervous system pathways. The stimuli which originate activation of the afferent nerve impulses may commence outside the body or within the body. These sensory inputs to the CNS would include the special senses and the general senses. Each of these sensory modalities can be related to specific end-organs, specific nerve fibers, and well-traced pathways through the spinal cord and brain stem. Most of these modalities are known by the names of the conscious sensations associated with stimulation of the end-organs.

In addition, there are bodily sensations of more complex composition which may involve, besides end-organs and sensory nerves, special centers in the CNS. These include hunger, thirst, respiratory need, urge to yawn, urge to sneeze, urge to micturate or defecate, sexual sensations, and perhaps others.

Thus far there is nothing that requires any designation more complex than sensation and awareness of it. All of the complex bodily sensations may increase in intensity to the point of displeasure or distress, and the removal of what is causing the sensation is accompanied by some sense of relief or comfort or pleasure. The simpler bodily sensations may also be associated with feelings of pleasure or displeasure. Awareness of distress and pleasure as accompaniments of bodily sensations requires a more complex neural apparatus and for the first time requires some psychological model providing for more than sensory perception. The activity may then be repeated for its own sake—that is, for the attendant sensations of pleasure.

It seems that recognition of this set of relationships was implicit in Freud's early models of sexuality and probably formed one basis of his concepts of sexual drives and ego drives. Activity is initiated by somatic changes and is terminated by somatic changes.

Developmentally, hunger, thirst, and certain sexual or sensual sensations require a partner; respiratory need, urges to yawn,

sneeze, micturate, defecate, and certain other sexual sensations do not.

None of these considerations of sensations of somatic tension/relief and subjective distress/pleasure apply to "aggression."

None of this—or very little of it—is psychoanalytic clinical data. It is somewhat imprecise neurology and does not depend upon the application of the particular method of psychoanalysis. Bodily sensations may be reported in analytic sessions. Bodily sensations that we would relate to aggression are generally perceptions of somatic changes in the fight/flight sequence—physiologic preparations for action.

If we start with psychoanalytic observational data and ask the same question that we did earlier—what is the relation between phenomena and the concept, this time, of aggression as a drive?—we can pursue the same line of thought, from inference of conscious awareness, data of conscious experience, inference of unconscious mental process and content with the characteristic of driven-ness. We cannot, however, give any answer concerning the impetus of aggressive trends in somatic terms.

In the case of sexuality the eventual criterion for the sexuality of a (mental or physical) behavior is the subjective awareness of a sexual feeling or sensation. Since the method of psychoanalysis is not introspection, but rather that of the psychoanalytic situation, this criterion can only be fulfilled via reports from verbal and willing subjects. For direct observation other criteria are necessary. Typical genital changes are adequate. Rhythmic manipulations of genitals or other body parts might or might not be acceptable as criteria (see Galenson [1971], discussed below).

In analysis we habitually make use of other, less unequivocal criteria. If a given behavior is not experienced as sexual by the reporting subject, but can reasonably be inferred to stem from some underlying impulse or fantasy which is the basis of known sexual behavior in children or perverts, we are willing to interpret the sexuality of the behavior and judge from the results whether or not the inference is correct.

In the case of aggression, the entire situation is less clear. Ideas and feelings involving anger, hatred, hostility, destructiveness, envy, spite, vengeance, cruelty, and so on seem to be warded off in the same way that sexual impulses are; they may be inferred, interpreted, and the effects judged just as is the case with sexual ideas and feelings. This is the psychoanalytic clinical evidence which Brenner, for example, sees as sufficient to justify the assumption of aggression as a drive. Aggressive elements (ideas, feelings, impulses) seem to be mentally similar to sexual elements: they are warded off and are a source of mental conflict.

We thus arrive at the conclusion that the psychoanalytic clinical evidence for aggression as a drive is, in this respect, very similar to the evidence for sexual drives, while the background of supporting, physiologic, and directly observable evidence present for sexuality is totally absent for aggression. In fact, the bodily changes associated with aggressive behavior are those accompanying the fight/flight series of reactions. The somatic referents for aggression seem to occur secondarily, as responses to environmental stimuli, rather than primarily as somatic stimuli leading to the search for an appropriate object (environment), and physiologically are of the general nature of preparations for action, unlike the specific changes seen in sexual behavior.

One of the significant sources of confusion about aggression is the failure to differentiate aggressive behavior, actual or mental, from aggression as an instinctual drive and from aggression as the hypothetical energy behind the drive. If this distinction is carefully made, one may, for example, raise the question: do all behaviors that are descriptively aggressive constitute expressions of an aggressive drive? A commonly held view is that the aggressive drive is intended to account for activity generally. While such a view might or might not be justifiable, it is in discord with the psychoanalytic theory of drives. The concept of drive in and of itself implies impulsion to activity.

There does seem to be widespread agreement that phenomena which may readily be called aggressive are important in human

behavior generally and in clinical situations, whether or not aggression is accepted as an instinctual drive. Marcovitz (Panel, 1979) suggested a "phenomenological classification of interpersonal behaviors that may be labeled aggressive": actions that are not hurtful so long as they are accepted without challenge (self-assertion, dominance, exploitation); hostility, which involves injury or destruction (removal of an obstruction to a goal, panic, defense); hatred, a distinctively human trait; sadism, the infliction of pain to provide sexual excitement (p. 656).

Whether or not Marcovitz's particular classification is accepted, some such phenomenological grouping would seem to be a necessary step (E. O. Wilson [1975], for example, offers a very different classificatory scheme). Phenomena observed in the psychoanalytic clinical situation might more fruitfully serve as a basis. In either event one must ask: what is the tacit basis of the classification?

Review of the observational data upon which we base the concept of aggression and of the relation of these data to the construct demonstrates that aggression is different from and not parallel to the libidinal drive concept so far as *source* is concerned. What about parallelism in terms of impetus, aim, and object?

Drive impetus is not much discussed under the term impetus. Yet the motivational impulsion to activity, reflected in the subjective sense of tension and relief, is the very essence of the drive concept. A. Freud (1972, p. 164) cited the "unmistakable *impetus* which is inherent in any aggressive striving; the unmistakable *relief* which follows its discharge; the unmistakable *distress* and its pathological consequences when discharge is blocked." We shall return to this aspect of aggression when we take up the relation of drives to the regulatory principles. The concept of aggression does seem to fit with the sexuality derived model of instinctual drives in respect to impetus; here aggression is parallel to libido.

What of the *aims* and *objects* of aggression? Anna Freud, viewing the problem developmentally, questioned the widely

held idea that "the aggressive drive itself . . . undergoes consecutive qualitative transformations" in parallel to oral, anal, and phallic sexual organizations (1972, p. 164). She noted major differences between sexuality and aggression in regard to both aims and objects:

> The libidinal aims, whether biologically prescribed, whether direct or sublimated are always specific to the drive. In contrast, aggression can associate itself with aims and purposes of extraneous kinds, lending them their force. We are familiar with this, of course, from the study of infantile sexuality. . . . Aggression also comes to the aid, either constructively or destructively, of purposes such as . . . vengeance, war, honour, mercy, mastery . . . i.e., in the service of aims which are dictated either by the ego or superego (p. 165).

Brenner (1971a) apparently agreed that aggressive aims cannot be treated in strict parallel to sexual aims. "Aggressive aims vary with mental development and experience. They seem to be related to what hurts or frightens a child" (p. 141).

A. Freud (1972) also pointed out changes, developmentally, in the instrumentality of aggression: teeth, excrement, the penis; the voice; the mouth for spitting; hands, arms, legs—the musculature generally; tools—knives, sticks, guns, bombs, poisons (p. 167). In relation to Hartmann, Kris, and Loewenstein's suggestion that the aims of aggression are more limited and rigid than those of sexuality, we might say that the instrumentality of aggression is more varied, plastic, or expansive.

Can the concept of *drive object* be maintained in the same way for aggression as for sexuality? (I shall consider the general question of the relation of drives and objects in the article that follows this one, pp. 414-426.)

In early infancy the development of sexuality and of aggression appears to be parallel with respect to objects, although our data are not particularly reliable for these stages. Spitz (1953) emphasized the role of aggression in establishing object relatedness. A. Freud (1972) observed that the developmental lines of sex and aggression become significantly different after

infancy. Hate, anger, resentment, and related feelings are more closely tied to satisfaction and frustration experiences than to a particular object: "there is no object constancy for aggression as there is for the libido" (p. 165).

Perhaps we can go further in specifying differences in libido and aggression with respect to objects. Freud's early idea of drive included a "specific action" which had to occur in the environment (object) to release drive tension. In this sense there is not even object specificity for aggression, with the possible exception that answering rage, like answering desire in sexuality, may be viewed as an object-specific drive tension releasing factor.

Besides divergencies between the concepts of libido and aggression in terms of drive source, aims, and objects, there are also differences with respect to defensive operations. Denial, repression, reaction formation, projection, identification, turning from object to self, and turning passive into active seem to be equally effective in relation to either drive. Identification with the aggressor, displacement of object from human to inanimate or animal, and undoing in obsessional neurosis seem to be directed against aggression only (A. Freud, 1972, p. 166).

The controversy which centered around the idea of life and death instincts has had unfortunate results: after sixty years clinical study of relevant phenomena and the formulation of clinical hypotheses about aggression are still in their infancy. Parallelism between aggression and sexuality is a temptingly convenient notion, but one which not only is not established but also is clearly impossible in many respects. Aggression requires at least somewhat different concepts, and it may be that the psychoanalytic concept of instinctual drive, worked out almost entirely on the basis of data on sexuality, has constrained observations on aggression, leading rather to unproductive theoretical disputes. Careful clinical studies, generating hypotheses with a minimum of intrusion by sweeping abstractions are still rare (see, however, Parens, 1979a). The idea of sexuality as an innate, somatically determined mental force

with tension buildup and discharge/pleasure modes is generally applicable to all phases of development. Ostensibly parallel ideas on aggression involve a cognitive appreciation or inherent awareness of the effects of aggressive actions upon objects, concepts clearly not applicable in a general sense (see Brenner, 1982).

When careful studies are more abundant, perhaps we shall choose to view aggression as something other than a drive. Perhaps we shall want to modify the drive concept, as Brenner does, to accommodate what we know about aggression. There are a number of other possibilities. Acceptance of the libidinal/aggressive universe of discourse has tended to obscure certain difficulties. The discrepancies between the concept of libidinal drives and the concept of aggressive drives are often disregarded. Distinction of the drives from the hypothetical energies behind them is often blurred or absent. A false impression of two qualitative forces with parallel vicissitudes tends to be created. Perhaps aggression must be viewed as a different kind of drive, which means that the psychoanalytic theory of drives requires modification to encompass aggression.

III: THE DEVELOPMENT OF DRIVES

Psychoanalysis has always been a developmental psychology, not only in the sense of studying and describing development, but also of seeing development in a genetic sense, as a cause, origin, or force determining, limiting, and generating later trends (Hartmann and Kris, 1945). Analytic theory has followed the device which seems to be standard in developmental or historical approaches, that of dividing a process continuous over time into phases or stages or periods. The phases are selected by the observer, usually or always with two objectives: differences over time in the selected developmental variable are highlighted; and the time variable, the chronology, is subordinated to the developmental variable, that is, the phase does not necessarily have to occur at precisely the same age in every individual.

Issues arise in a developmental or genetic approach in the following areas: (1) the application of the phase concept—how does one decide what a phase is to consist of?; (2) the sequence of phases; (3) the content of each phase; and (4) the relation of the selected variable to other aspects of, in this case, personality or behavior for each phase.

In considering the development of drives, we are continuing to deal with an inferred variable, that is, a theoretical construct, now with one additional level of complexity introduced by the developmental perspective, and a second by the postulate that psychological regression as well as progression may occur. Phenomena partially explainable by the drive construct are compared at different ages, isolating the drive construct as necessary for such comparisons. A methodologic difficulty also arises, in that direct observation of children, as opposed to the psychoanalytic method, necessarily plays a greater role. Problems of integration of propositions arise because of the different method and the different observational base.

The issues of the theory of instinctual drive development range from philosophical to methodologic to political. How, again, do we account for the ontogenetic origin of mind? Psychoanalytic clinical data on very early stages of development are not reliable. We must turn to direct observation of infants and children to make or confirm hypotheses about drives in these phases. How do we integrate hypotheses derived from direct observation with those derived by use of the psychoanalytic method? This issue, especially, focuses the question of criteria for both direct observation and clinical psychoanalytic data. Issues concerning drive development in girls and sexual function in adult women have also remained controversial. Has recent work, clinical or by direct observation, brought about changes in the theory of drive development in females?

Freud postulated a sequence of developmental phases of the sexual drives, each named according to a predominating erotogenic zone and a mode of object directedness: oral-cannibalistic,

sadistic-anal, phallic-oedipal, and, after an intervening period of relative drive latency, genital. In each of these developmental phases the scopophilic/exhibitionistic and sadistic/masochistic component drives are apparently organized according to the predominant zone and mode (see Compton, 1981a, 1981b). Each of the developmental phases also includes bisexual trends, but these are not clearly distinguishable in Freud's work until the phallic-oedipal phase in which the positive (heterosexual) and negative (homosexual) oedipal attitudes are recognized. The fate of what Freud called "homosexual libido" in the genital stage was less clear but was related to the formation of the "ego ideal."

While the basic elements of this sequence of sexual drive organizations were not significantly altered in Freud's later work, after the introduction of the ego-id-superego model drive development overall began to be viewed somewhat differently, in relation to the structural differentiation of the mental apparatus. This was reflected in the concept of the undifferentiated phase, introduced by Freud in 1938 (see Freud, 1940) and developed by Hartmann (1939) and Jacobson (1954, 1964) (see Compton, 1983, pp. 402-408). Hypothetically, once ego-id differentiation has commenced, the notion of drive organization begins to apply, and the developmental sequence of drive organizations is an aspect of id development. (Recall once again that these terms all designate theoretical constructs, such that phenomena are to be explained in terms of the interaction of the constructs.)

These ideas about steps from an undifferentiated phase to the beginnings of mental structure and mental forces in the form of drives relate to "getting the mind started" developmentally, in a highly abstract fashion. In an attempt to come closer to phenomena, Schur (1966), following his earlier ideas on desomatization (1955), proposed that the mental apparatus of the newborn does function according to the model of a reflex apparatus. A transition or progression then occurs, from reflex to wish. This is the model for the transition from somatic

needs to instinctual drives as mental representations of stimuli of somatic origin. This transition also marks the development of the structure id from the undifferentiated phase (and presumably of the earliest form of the ego as well).

Although methodologic and conceptual problems make the integration of observations with these hypotheses hazardous, Dowling's (1977) work with infants afflicted with esophageal atresia apparently presented observations relevant to the developmental sequence that Schur postulated. In these infants the gastrostomy separates nutritive need from oral experience. In one case particularly (pp. 230-233), development from reflex sucking and swallowing to purposeful, motivated oral demand could be seen very clearly.

The developmental differentiation of psychic structure and the concomitant and ensuing developmental sequence of libidinal organizations are usually seen as the result of a genically encoded program (Brenner, 1955, p. 17). Developmental changes in drive organization are explained at a psychological level as a detachment of libido from an object and/or mode of gratification, and cathexis of a new object and/or mode (p. 28). The work of Dowling and others clearly indicates that the unfolding of the genic program depends upon the opportunity for zonal interaction with the environment. In terms of classical drive theory, the availability of an erotogenic zone and of a suitable object partially determines drive development.

Since the 1949 publication by Hartmann, Kris, and Loewenstein, which proposed some degree of parallelism between libidinal and aggressive drives, Freud's sequence of libidinal organizations has generally been taken as a sequence of drive organizations, with all of the component drives having aggressive as well as libidinal aspects. Propositions specifically concerning the mechanisms of advance of aggression are, however, far less clear or wanting altogether. I noted above some questions concerning the parallelism and the kinds of data offered in support of these hypotheses.

Freud's proposals on drive development have been modified

in a number of other significant ways. Many of these modifications also derive from the direct observation of children. Mahler (1966) mentioned the child's discovery of anatomical sexual differences at a much earlier age than had been previously recognized—fourteen to sixteen months. Roiphe and Galenson have been pursuing this suggestion in a series of ongoing studies, amply confirming and expanding the observation (Galenson, 1971; Galenson and Roiphe, 1971, 1976, 1980; Roiphe, 1968). They proposed an "early genital phase" as a normative developmental event, starting between sixteen and nineteen months, during which "the genital zone emerges as a distinct and differentiated source of endogenous pleasure. . ." (1976, p. 54). This early genital phase is associated with discovery of anatomical sexual differences and a "preoedipal castration reaction," which becomes fused with earlier fears of both object and anal loss and also influences the later phallic-oedipal phase as well as other aspects of development. Obviously, boys and girls have different reactions to this early genital phase and show significant differences in subsequent psychological development.

These studies are pertinent to proposals for modification of Freud's hypotheses on female sexual development. We have seen that Freud came to the study of male/female differentiation in childhood only after 1925 (although he was clearly aware of such differences in early infancy in 1905 [pp. 188-189]) and in connection with his interpolation of the phallic-oedipal phase into the developmental sequence (Compton, 1981d). Freud's propositions did not systematically extend the idea of male/female differentiation to younger ages or developmental phases earlier than the phallic-oedipal.

Freud's ideas on female sexual development and function (1925, 1931, 1933) aroused controversy (a not unusual result of Freud's ideas!) from the time of their introduction (Horney, 1923; Jones, 1927; M. Klein, 1928). Phallocentric bias against women is held to invalidate psychoanalytic views of women, then extended to dismiss instinctual drive theory and psycho-

analytic psychology in general. Given the essential ideas that girls, in the course of their development, must change their primary love object from mother to father, must change their leading erotogenic zone from clitoris to vagina, and have a different relation of the castration complex to the oedipus complex than boys do, what are the issues that are raised? First, are girls not different from boys from the beginning? If so, what relation does this have to the instinctual drive development of girls in the preoedipal years? Second, does a change in the "leading zone" from clitoris to vagina actually occur? Is there "early vaginal awareness" and, if so, what effect does this have on the leading zone hypothesis? What bearing do "types of orgasm" in women have on this proposition? Finally, if a change in the primary love object occurs, how does this come about?

These issues must be viewed in the context of the psychoanalytic theory of drive development in general, and especially the theories of genital primacy and of preoedipal development. Freud's 1905 concept of genital primacy was hardly that of orgasm location. The changes from infantile to genital sexuality were held to include: (1) transition from autoerotic to object-directed predominance; (2) transition from separate or loosely organized drives and zones to a thoroughgoing genital taking over—"fitting in"—of all sources of excitation; (3) change from general pleasure as an aim to a specifically sexual, genital aim; and (4) convergence of sexual and affectionate currents on the same sexual object and sexual aim (1905, p. 207).

Similarly, Freud's concept of preoedipal development was not simply another name for the oral and anal phases. It entailed a retrospective view, from the standpoint of the oedipus complex, upon how that set of drive and object orientations came into being out of the background of oral-cannibalistic and sadistic-anal organizations and corresponding phases of ego development.

Differences in the development of boys and girls began to become clearer in relation to the preoedipal view. For boys the question is: of what does the change in attitude toward the

retained preoedipal object (mother) consist? Loewenstein (1935) and Lampl-de Groot (1946) answered this in terms of the boy's turning from passive phallic aims to active phallic aims.

For girls, the issues, as suggested above, extend to earlier developmental phases.

Since the 1920's a number of authors (e.g., Horney, 1923; Jones, 1933; Zilboorg, 1944) have argued the case for "primary femininity" and, in the case of Horney, primary heterosexuality. (It seems fair to say that heterosexuality is seen classically and currently by most analysts as a developmental achievement, a product of mental differentiation and not only of physiology.) Stoller (1968, 1973, 1975, 1976, 1979) has recently discussed the same idea of female sexual development in his concept of "core gender identity," the sense of maleness in men and of femaleness in women (primary femininity), present as a developmental consideration continuously from prenatal life. Stoller proposed that this gender sense depends upon: biologic forces organizing the fetal brain; sex assignment at birth; the mother's attitude toward that sex; the effects of early postnatal handling patterns; the developing body ego, especially defining effects of genital sensations; and mental development.

There seem to be two sets of issues here. The first concerns early developmental phases. As Stoller pointed out, sexuality is not the same as gender sense, and neither is the same as object choice. The construct of instinctual drives is intended as explanatory of, among other things, sexuality and, in part, of object choice. What is the relation of the drive concept to the concept of sense of gender? The pregenital drive organizations would seem to be not much related to early gender sense. Gender sense and instinctual drive would come into relationship only when the drive organization has advanced to a genital —that is, phallic or early genital—stage. On the other hand, early vaginal awareness would indicate unconscious representation as well and would necessitate some modification of Freud's concepts of pregenital organization in girls to include such representation. This might or might not have the effect of

establishing a "feminine developmental line," implying that vicissitudes of drives and their derivatives are different for boys and girls at every developmental phase.

Lack of awareness of the vagina by both sexes in childhood was an important element of Freud's picture of the phallic-oedipal phase. Bonaparte (1935) postulated early vaginal awareness, but attributed it to an overflow of rectal and clitoral sensations in her "cloacal theory." Greenacre (1950, 1953, 1958) presented psychoanalytic clinical evidence for vaginal awareness in girls from early infancy onward. Kestenberg (1956a, 1956b, 1968) also presented evidence of early vaginal awareness and suggested a far-reaching influence of this awareness in maternal behavior and adult female sexual function. Fraiberg (1972) made observations on the same topic and added a collection of cases of masturbatory orgasm, or something close to it, in girls ages four to nine.

Early vaginal awareness with unconscious representation of vaginal tensions, the persistence of masturbation in latency girls, and the presence of prepubertal orgasm-like responses all would seem to require alterations of or additions to Freud's view of female sexual development.

The second set of issues has to do with suggested modifications of later infantile phases of girls' sexual development, not based on the concept of primary femininity or the observation of early vaginal awareness.

Galenson and Roiphe (1976) noted the following divergences in connection with the early genital phase and the child's discovery of anatomical differences. In girls, there is an increased investment of fantasy life, a change in basic mood, an alteration of masturbatory patterns (away from manual), and a turning to the father "in a newly erotic way, seeking the mother's attention only during periods of distress." Boys, in contrast, showed persistence of manual masturbation, increases in general motor activity, and less tendency to fantasy play and graphic representation (pp. 49, 51). They concluded, "We believe that Freud's original position was partially correct in that penis envy and

the feminine castration complex do exert crucial influences upon feminine development. However, these occur earlier than he anticipated . . . and they shape an already developing, although vague sense of femininity" (p. 55).

Grossman and Stewart (1976) seem to be approaching the same developmental considerations from the viewpoint of psychoanalytic clinical study of adult women. They postulated two phases of penis envy. The first occurs as a narcissistic injury, which may be resolvable and may contribute to female development, or become one of a series of envy-producing traumas in a narcissistic character. The second phase is a regressive attempt to resolve oedipal conflicts.

The debate over the change of the girl's leading erotogenic zone seems to have been carried out on false premises. In the first place the data were obfuscated. Glenn and Kaplan (1968) pointed out that "clitoral orgasm" and "vaginal orgasm" appeared in the literature without definition as terms and may mean: (1) the area stimulated to bring about orgasm; (2) the area in which the orgasm is subjectively experienced; (3) the area in which physiologic changes occur in the genitalia; and that a variety of combinations occur. To this it should be added that Freud's theory did not include the terms clitoral or vaginal orgasm, but referred to libidinal cathexis of (the representations of) anatomical regions.

The developmental issue of the phallic-oedipal phase in girls has been summarized in this way: "The issue that needs to be examined is whether or not heterosexual object choice in women is tied to a reaction against 'active aims' in sex" (Grossman, 1981).

Issues regarding the roles of narcissism, masochism, and passivity in female development are less clear. The idea of a primary erotogenic feminine masochism has also been considerably challenged (see Blum, 1976; Brenner, 1959).

The well-known and widely respected work of Margaret Mahler would seem to have bearing upon psychoanalytic theories of drive development. Integration of the two bodies

of hypotheses has, however, proven difficult and seems unsatisfactory in most respects at this time (see, however, Parens, 1979a, 1979b).

In summary, Freud's hypotheses regarding a sequence of developmental phases of instinctual drives remain as the basic psychoanalytic theory of drive development. The earliest phases of drive development are now viewed in relation to the onset of differentiation of psychic structure. Developmental phases now tend to be seen as drive organizations rather than only libidinal organizations, although this formulation is open to serious question. The child's discovery of the anatomical differences between the sexes is now recognized as being significant at a considerably earlier age. Sexual behavior in the latency period has been found to be more common in both boys and girls than Freud thought. Issues of female sexual development remain controversial.

The implications of much of the recent observational work upon the general theory of instinctual drives and drive development remain to be determined.

SUMMARY

The evolution of Freud's theory of instinctual drives, with the accompanying models of a mental apparatus, is remarkable for its tenacious adherence to addressing the fundamental problems of human psychology, here phrased as the problems of body-mind-environment relationships.

The concept of instinctual drives continues to be one of the most pervasive concepts of psychoanalysis, weathering considerable attack over the last several decades, although losing some clarity in the process. I have cited and discussed as basic issues of the concept of instinctual drives: the relationship of observational data and theoretical constructs in psychology; whether our construct of drives is or should be or can be purely psychological; the problem of conceptualizing the ontogenetic origin of mind; the issues of the "force-meaning conjunction" and the

problem of psychic energy in psychoanalytic constructs; and the relation of our concept of instinctual drives to the concept of instincts in general. It seems that progress with these fundamental issues might be made by utilizing models that are more homologous with present knowledge in related fields than is Freud's reflex arc model of the nervous system, in order to build a better drive construct within the framework of psychoanalysis.

The classification of instinctual drives remains a problem. Clinically, aggression seems to be a factor in conflict, very much like sexuality. Despite widespread acceptance of the idea of aggression as simply parallel to sexuality in all respects, there are major discrepancies. Perhaps aggression cannot be viewed as a drive after all; perhaps our drive construct needs to be modified to accommodate aggression. Certainly, controversy in this area has interfered with the production of good clinical studies which could begin to increase our understanding of aggression and its place in the human personality.

The psychoanalytic theory of drive development has probably undergone less change in the last forty years than other aspects of drive theory, although careful observational work has led to alterations of our views of the age of onset of genital awareness, of female sexual development and function, and of the latency period.

This is the conclusion of the first of two papers examining the post-Freud development of the theory of instinctual drives in psychoanalysis. The companion paper follows on p. 402 of this issue of *The Psychoanalytic Quarterly*.

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The Current Status of the Psychoanalytic Theory of Instinctual Drives

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THE CURRENT STATUS OF THE PSYCHOANALYTIC THEORY OF INSTINCTUAL DRIVES

I: DRIVE CONCEPT, CLASSIFICATION, AND DEVELOPMENT

BY ALLAN COMPTON, M.D.

This paper proceeds from the basis of a scrutiny of the evolution of Freud's drive theory to survey and synthesize the major trends in and problems of recent and current contributions to the psychoanalytic theory of the concept, classification, and development of instinctual drives.

The relationships between body, mind, and environment present a central and unavoidable problem for any serious psychology. Historically, in psychoanalysis the theory of instinctual drives has been the primary conceptual device for dealing with these relationships. More recently, the propositions of ego psychology have added a complementary set of concepts which also deal with mind-body-environment interactions.

Today the concept of instinctual drives continues to be one of the most pervasive concepts of psychoanalytic literature, even in the writings of those authors who are highly critical of drive theory. Virtually every psychoanalytic paper utilizes or has bearing upon some aspect of the theory of instinctual drives. Despite this pervasiveness—or perhaps because of it—there have been very few attempts to scrutinize drive theory as a whole, to review its development, to summarize changes, or even to piece out the relationship of the elements of drive theory to one another. The standard reference remains Bibring's (1936) review of Freud's work (see also, Arlow, 1959; Brenner, 1955, 1982; Fenichel, 1945; Nunberg, 1956; Waelder, 1960).

The purpose of this study is to trace the development of the psychoanalytic theory of instinctual drives in the last forty years. The formulation that there are three phases of psychoanalytic drive-structure theory that are incompatible in certain respects (Compton, 1981a, 1981b, 1981c, 1981d) will be used to understand these post-Freud contributions. To what degree has our concept of drives changed? Have any of the problems inherent in Freud's drive theory been solved or reduced? Have any been magnified or enhanced? What difficulties in or advantages of psychoanalytic drive theory have emerged?

A comprehensive survey of the recent literature on instinctual drives is not intended. I shall instead be concerned, in this paper and in the companion paper that immediately follows it, with those contributions which have particularly helped to focus the ongoing issues of drive theory. In this paper my discussion will be organized in the following sequence: (1) the concept instinctual drive; (2) the classification of instinctual drives, including the theory of aggression; and (3) the development of drives.

I: THE CONCEPT INSTINCTUAL DRIVE

In Freud's most extended definition of instinctual drive (1905 [1915], p. 168), he said that a drive is the "psychical representative" of a force, pressure, or demand that arises in the body as excitation in an organ. Its aim is the removal of excitation in that organ. Instinctual drive is a frontier concept, to be understood as a demand for work made by the body upon the mind and in itself is without quality. This concept of instinctual drive, the one with which psychoanalysis grew up, clearly emphasizes the relationship of mind and body as the central issue.

Freud also proposed (1915a, pp. 122-123) that any instinctual drive might be characterized according to its source, impetus, aim, and object, adding (p. 123) that exact knowledge of the source is not necessary for psychological investigation and is, in fact, outside the scope of psychology. In this statement Freud

explicitly recognized a problem that arises for psychoanalysis: observational data are primarily psychological; a core concept has a central somatic referent; there is built-in ambiguity in mind-body terms.

Most of the basic issues of the concept of instinctual drives can be approached on the basis of these statements by Freud. In listing these issues, I shall necessarily include most of the fundamental problems of human psychology. That necessity simply reflects the nature of the psychoanalytic concept of drives.

(1) What is the relationship between drive as a concept and the data of observation?

(2) In those areas which involve "interactions" between body, mind, and environment, how are the conceptual gaps and discontinuities to be dealt with? Does the nature of the observational data of psychoanalysis necessitate doing without somatic referents in the formulation of theoretical constructs? What is the effect of formulating constructs in a "purely psychological" way?

(3) How is the ontogenetic origin of mind to be accounted for in the context of drive theory?

(4) Is it possible to state meaningful hypotheses which include both force terms and meaning terms?

(5) What is the relation of the psychoanalytic concept of instinctual drives to the biological concept of instinct?

(6) If one accepts the construct of mental systems, thus aligning drives with one of the systems, drives must then be viewed theoretically in terms of interactions between the systems. Do we do this successfully? Is it of value?

Understanding the first issue is a prerequisite for the rest. *What is the relation of the concept of drive to observational data?* For the moment I shall use Freud's pre-1920 formulations as a context, and sexuality as the sample drive, returning later to modifications necessitated by the ego-id-superego model and aggression. The question may be approached clinically or theoretically; the reasoning is different.

Clinical approach. As Freud pointed out (1915c), the primary data of psychology are the elements of conscious awareness: thoughts (ideas), feelings, perceptions (including sensations), and a sense of more or less—more or less clarity, vividness, value, strength, urgency, intensity, and so on. The method of psychoanalysis is not, however, introspection. Psychoanalytic method depends upon the inference that other people have a consciousness similar to one's own. Given that inference and the psychoanalytic method of observation of (reports of) mental process (conscious content), gaps in conscious awareness are abundantly evident. We then make a second inference: there must be unconscious mental processes; we do not and cannot know them, but say that they consist of elements such that, if they were conscious, would be thoughts, feelings, and a sense of more or less. Using the second inference, we "discover" certain kinds of processes and contents to be characteristic of the unconscious part of the mind—urges (or impulses or wishes or fantasies), tending to be sexual or aggressive in their inferred content and traceable to childhood. We now ask: what gives these elements their apparent impetus? And we make a third inference, that the impetus stems somehow from particular somatic connectedness or somatic sources. Thus, the concept of instinctual drives is inferred from clinical data in three distinct steps. Certain elements of behavior, including physical behavior and conscious awareness, are then said to be partially explained as results of derivatives of drives. Instinctual drive is thus an explanatory construct and one which depends upon the inference of unconscious mental processes. An instinctual drive is never seen as behavior, nor is a drive ever manifest as such in consciousness. The data of conscious awareness are thoughts, feelings, perceptions, and a sense of more or less, not drives.

Theoretical approach. An instinctual drive, as defined by Freud, may be said to occur as a result of somatic demands for work in the form of unconscious wishful impulses seeking satisfaction. Derivatives of these wishful impulses—drive derivatives—may be involved in the formation of unconscious fanta-

sies and, in the form of further derivatives, may become conscious as ideas, feelings, and a sense of more or less. From this approach the primary data of consciousness may be seen as dependent variables, partially determined by drive derivatives and, eventually, by drives. Drives are seen as, approximately, independent variables within the mental systems. Again there is no implication that drives themselves are ever directly observable. Statements about drives are not statements about physical or mental behavior. Instinctual drive is an explanatory construct.

Does the nature of the observational data of psychoanalysis, and the relationship of that data to theoretical constructs, necessitate doing without somatic referents in the formulation of theoretical constructs such as that of instinctual drives? Must we create a "purely psychological" concept of drive?

In recent years there has been a distinct trend toward purely psychological concepts of drive. Brenner (1955), for example, said that a drive is

> a genically determined, psychic constituent which, when operative, produces a state of psychic excitation or . . . tension. This excitation or tension impels the individual to activity. . . . This activity should lead to something which we can call either a cessation of excitation or tension, or gratification (p. 17).
>
> [Drive] does not include the motor response but only the state of central excitation in response to stimulation (p. 16).

While this definition does not exclude the mind-body relationship, that element is far less prominent than in Freud's definitions. Brenner, in more recent work (1982), specifically eschewed the "frontier" aspect of Freud's definition (see below).

Loewald (1971), who preferred the term "instinct" and discussed instincts in the context of the concept of motivation, was very explicitly purely psychological:

> . . . I consider instinct a psychoanalytic psychological concept, which should be kept free of biological and ethological connotations. . . (p. 105).

> [Instinct] in psychoanalysis . . . is a mental force or stimulus . . . the most primitive element . . . of motivation . . . . a motivational stimulus, constituent of the stream of mental life, and not a biological stimulus operating upon that stream (p. 109).

Loewald chose to emphasize Freud's phrasing that "instinct" is the "psychical representative" of a physiologic stimulus: what the organic stimuli stimulate is the "faculty of representing, an activity which is then seen as inherent in the mind. . ." (pp. 107-108).

Loewald has, in his effort to be purely psychological, run aground on one of the basic issues of drives, the *problem of the ontogenetic origin of mind.* This problem may be profitably viewed longitudinally or cross-sectionally. In the longitudinal sense, the human organism develops from the fertilized egg to the physically and psychologically mature adult in an infinite number of steps that can be variously organized. But how or when does mind enter into this developmental sequence? In the cross-sectional sense, at any given moment, after some particular developmental point, we see somatic processes as somehow or other reflected in certain aspects of what we study as mental processes. As Brenner said (e.g., 1982), mind is a function of brain. But how, at any given moment, does this occur?

These questions are not usually made explicit in the psychoanalytic literature, but the positions tacitly taken which serve to answer them are the apparent source of many nonproductive disputes. The terms of these positions tend to be treated as "fundamental indefinables" of our science, which they are not. Some examples are: Freud's phylogenetic precipitates; perhaps Freud's concept of psychic energy; apparently inherited unconscious fantasies (Jung, M. Klein), which are present at birth and form the "deepest layer of the mind"; the strange word "mentalization" which has been appearing in recent literature, as "internalization" becomes more recognized as problematic; "internalized object relations" (Kernberg, 1976, e.g.). Loewald's "faculty of representing" is another such position: it does not

improve Freud's formulations about drives, but only buries the question of the ontogenetic origin of mind. Mind is, in Loewald's formulation, apparently synonymous with the "faculty of representing," or the faculty of representing is how anything, including a drive, can become "mentalized." Rather than the drive coming to the mind and demanding work, the faculty goes to the drive and creates representation. Ambiguity and discontinuity exist in both formulations. In this circumstance, explicit ambiguity, as in Freud, seems more propitious.

Rapaport (1960) seems to have introduced to the psychoanalytic literature the attempt to place drives within the context of "motivation" as a more general concept. He proposed that all behavior is causally determined, but not all behavior is motivated; that is, motives should be distinguished from other causes. Other causes may be classified as physical (mechanical), somatic, and social (pp. 862-863). He defined motivation as an appetitive force, not equated with any specifiable physiologic process, experienced by the subject either as actively willed or passively suffered. Appetitiveness, the key word, is characterized by pre-emptoriness, a cyclic pattern (leading to consummatory activity), selectiveness, and displaceability (p. 865). Selectiveness, he said, implies "that the direction of the motive force is determined by its object . . . . there are objects which are the specific necessary conditions for the discharge of the energies of the motivations in question" (p. 865). These definitions are apparently intended to allow for psychic forces which are nonmotivational (as well as to provide a set of purely psychological terms for defining drive). Defenses, for example, are not appetitive, have no object, and are not motives (p. 866).

Rapaport acknowledged that what he had done was to give instinctual drives another name—motivations (pp. 866-867). Apparently, he wanted to avoid foreclosing the possibility of nondrive motivations, as well as to allow for nonmotivated determinants of behavior, that is, causes. He then went on in an attempt to improve the concepts of Freud's drive theory, unfortunately also attempting to ascribe the improvements to Freud.

Rapaport was trying to show that Freud's drive theory is purely psychological (pp. 879-880), which it clearly is not at any of the steps in its evolution, saying that the attempt to discuss sources leads to "the impression of a primitive peripheralist theory of instinctual drives" (p. 880). If somatic sources are not to be discussed in the theory, how does one deal with, for example, the sequence of somatic changes in sexual excitement and release? Rapaport said, "The direct manifestations of an instinctual drive are the observable consummatory actions. The other effects of instinctual drives are termed instinctual drive representations (or presentations)" (p. 881).

In the last statement Rapaport seems to be in trouble with both the relation of the drive concept to observational data and the mind-body conceptual gap. Among the causes of (physical and mental) behavior are motivations, defined as appetitive forces not equated with any specifiable physiologic process. Among the appetitive forces—in fact, the only specimen of them—are instinctual drives; Rapaport merely exchanged words. Instinctual drives have mental effects, which are termed "drive representations," and other effects, termed "direct manifestations," which are the "observable consummatory actions"—that is, physiologic processes. Rapaport, in effect, reintroduced and buried the ambiguity he was trying to eliminate and added a statement about direct manifestations of drives which is at odds with the conceptualization of Freud and most other analysts.

More recently Stoller (1979) has also opted for a purely psychological theory, in this case of sexuality or, rather, sexual excitement. He elected in his discussion of the generation of sexual excitement, to "put aside" special physiologic factors and direct stimulation of body parts (pp. 6-7). Later he said, "We shall never make it work with a pseudoanalysis (metaphysics) of psychic energy; rather, psychoanalysis is the study of, the search for, meaning" (p. 193). Stoller gave an extended and valuable report of the role of sexual fantasies—"scripts"—in the erotic life of his patient, attempting to reduce the role of "metaphoric usages" in psychoanalytic theory (referring chiefly

to psychic energy). At one point, however, he said, "It is the nature of mucous membranes to be charged with potential for that form of pleasure we label erotic. The quality of the sensations is ultimately of physiologic origin and so is hard to deny" (p. 107). Stoller would seem to be back in the same dilemma: mind-body ambiguity masked by (a new) metaphor, "charged with potential." In this respect Stoller's work typifies that of many others who strive to take atheoretical approaches: unacknowledged theoretical assumptions creep into formulations masquerading as pure description or metaphor, and scrutiny reveals the formulation to have fallen upon one or another of the basic issues of drive theory.

G. S. Klein (1976a, 1976b), Gill in his more recent work (for example, 1976), and Holt (1967, 1976), among others, also tried to work out purely psychological theories of sexuality or of drives generally, and to divorce psychoanalytic theory entirely from concepts of mental energies.

Brenner (1982) has recently developed his ideas further, in a direction which attempts to obviate some of these difficulties. He took the position that the weight of psychoanalytic—that is, clinical, psychological—evidence is strongly in favor of libido and aggression as driving forces in mental life, but that the same cannot be said of the mind-body frontier concept of drives.

> All of psychology is an aspect of the functioning of the central nervous system. There is no frontier between mind and body . . . mind is an aspect of cerebral functioning. . . . Since all mental functioning is somatic, it is unnecessary and, in some respects, even misleading to separate one group of psychological phenomena from others by labeling them as somatic in their nature (pp. 19-20).

What, then, of the relationship between libido and the erotogenic zones?

> It is important to emphasize the following with respect to libido, however. Libido is closely tied to and influenced by what Freud called the erogenous zones as well as by various sexual metabolites. . . . Nonetheless, it is worth repeating that there

> is no convincing evidence that the erogenous zones are, in fact, the sources of the libidinal drive. To be sure, wishes for satisfaction of one sort or another are profoundly influenced by many factors, among which stimulation of the erogenous zones figures largely. . . . However, such stimulation, hormones, etc., are not sources of libido. . . . Libido, like all other psychic phenomena, including aggression, derives from the functioning of the brain. It is one of the features of the aspect of brain functioning we call the mind (p. 24).

Where does this leave us? Certainly, the peculiar notion that some special excitation is somehow transmitted from the erotogenic zones to the mind, to which I have repeatedly objected (Compton, 1981a, 1981b, 1981c, 1981d), is eliminated. Perhaps it is satisfactory to allow it to stand, for the time being, that the problems of the relation of erotogenic zones to mental forces are problems for neurophysiology and for the study of neurohormonal interactions—or at least that it is impossible to say more. Brenner has, in any event, stopped short of inventing a phrase which has the effect of obscuring a basic problem. He has also remained within the confines of his own rigorous definition of drive as the producer of a state of psychic excitation. He provided a clear somatic referent in saying that mind is an aspect of cerebral functioning.

We can, however, raise the same question here. Does the nature of psychoanalytic data necessitate purely psychological formulations of the construct instinctual drive? It would seem to be the essence of Freud's drive concept that certain physiologic processes which commence, or may commence, outside of the central nervous system have a special relation to psychological phenomena, mediated, of course, through structures and pathways of the central nervous system. And, at least where sexuality is concerned, it is this special relation that is intended to account for the driven-ness. Something essential would seem to be lost by eliminating this relationship as an integral factor in the explanatory construct of drives.

Thus far, I have been discussing contributions affecting the

psychoanalytic concept of instinctual drives in the framework of the first three basic issues I listed—concept/data relationship, body/mind/environment relationship, and the problem of the ontogenetic origin of mind. All of the basic issues may be seen as facets of one comprehensive problem, and Ricoeur (1970), although he was not focusing on the concept of instinctual drive and approached issues from a very different viewpoint, grasped, I believe, very clearly, another of the facets when he said,

> What is the status of representation or ideas in relation to the notions of instinct, aim of instinct, and affect? . . . How can the economic explanation be *involved* in an interpretation dealing with meanings; and conversely, how can interpretation be an *aspect* of the economic explanation? It is easier to fall back on a disjunction: either an explanation in terms of energy, or an understanding in terms of phenomenology. It must be recognized, however, that Freudianism exists only on the basis of its refusal of that disjunction (p. 66).

He made a similar point in 1977: "in short, practice forces us to think of meaning and force together in a comprehensive theory" (p. 850; see also, p. 856). Freud's insistence on propositions that are ambiguous in mind-body terms represents this same recognition of the necessity for a conjunction of meaning and force.

We have noted thus far two factors in the search for purely psychological concepts of instinctual drives: respect for the nature of observational data and the advantages of circumventing the mind-body problem. Ricoeur's observations bring into focus a third factor: dissatisfaction with the concept of psychic energy is connected with dissatisfaction with the somatic aspects of the concept of drive because energy was one of the main ideas in which Freud housed the mind-body ambiguity. It was also Freud's device for "getting the mind started," and, in Ricoeur's terms, refusing the force-meaning disjunction. While there is much to be said concerning the disadvantages of the concept of psychic energy and how we use it, I agree

with Brenner (1980) in this respect:

> . . . in that theory there should be some term to designate the concept that drives have a capacity to impel the mind to activity —a capacity that varies in strength from time to time. What that concept is *called* matters not at all. . . . Just changing the *name* from "drive" to, say, "motivation" changes nothing in the theory. It makes the theory no more "psychological," no less "mechanistic," no more "human," than it was before (p. 211).

It might be added that, on empirical grounds, there must be some conceptual device to account for the sense of more or less.

A related issue has appeared as a terminological problem: whether to translate "*Trieb*" as "instinct" or as "instinctual drive." The issue, of course, is the relation of the psychoanalytic concept of instinctual drives to the biologic concept of animal instincts. Hartmann (1948), preferring "instinctual drive" to emphasize the difference between the two concepts, cited the following characteristics which differentiate human drive-related behavior from animal instinctive behavior: relative independence from outer stimuli, leading to greater plasticity of adaptive behavior; greater variability of responses to inner stimuli; continuity of driving forces (pp. 78-79). Brenner, too, clearly differentiated instinct from drive, emphasizing the state of central excitation as the drive, with the quality of plasticity ascribed to mediation of motor response by ego functions (1955, p. 16; see also, Waelder, 1960, pp. 97-102).

The biological concept of animal instincts has also changed (see Fletcher [1957] for a survey of usages, and Wilson [1975] for an approach to the same phenomena essentially minus the instinct concept), and presently bears little resemblance to Freud's tension reduction/reflex arc model of 1900. Kaufman (1960), for example, made a very convincing case for the idea that instinctive behaviors in animals are interrupted by certain stimuli—*terminating stimuli*—rather than by a discharge of specific energy. An example is a bird that normally lays four eggs but continues egg-laying if eggs are continually removed from the nest so that less than four are present.

Such studies raise significant issues for psychoanalytic drive theory, even though the concepts are certainly not directly transposable. If a carefully phrased definition such as Brenner's, in terms of "psychic excitation," is accepted, what would be the effects of using a cybernetic model, rather than a reflex arc/discharge model, as a basis for the drive concept? I have suggested elsewhere (Compton, 1981a) that Freud's early "Schematic Picture of Sexuality" (1895a) had a more prominent feedback loop quality than his later drive theory. Hartmann (1948, p. 72) has pointed out that, historically, the study of aims and objects became far more prominent in psychoanalysis than the study of sources. A cybernetic model of drives might afford a more balanced recognition in the theory for this historical trend of increasing psychological emphasis, and also permit further de-emphasis of drive-specific energies while not necessitating a detachment of psychological from somatic functions. Other problems are, however, readily introduced.

Considering the concept of instinctual drives leads into a number of issues in addition to those formulated above as questions. Many of those additional issues and the question concerning the structural position of drives will be discussed in the sections following. It is not my intention here, however, to try to resolve that list of weighty, fundamental problems which the drive concept of psychoanalysis brings into some degree of focus—or even to resolve the less formidable issues to be noted subsequently. The work of Brenner, Hartmann, Loewald, Rapaport, Ricoeur, and others has served to clarify some of the problems in Freud's drive theory, whether or not we find their proposed solutions entirely acceptable. The recurrent idea that a "purely psychological" drive concept can obviate some of these difficulties is tempting on methodologic grounds, but it creates other problems and seems to do violence to Freud's fundamental drive concept.

Various axiomatic positions have been taken, historically and currently, which foreclose most of the fundamental issues cited here. Such positions do not resolve anything and lead to unpro-

ductive theoretical disputes. Solutions to the fundamental issues which drive theory brings to light must be approached with skepticism. There is, however, no reason not to make use of the current, more accurate, and sophisticated models in neurophysiology and ethology, and no reason not to utilize new concepts derived from cybernetic and information theory, or even sociobiology, to alter and improve psychoanalytic drive constructs, and to strive for greater homology with data from other avenues of approach to behavior. Such efforts must be differentiated from translations of old concepts into new terms which "sound better" but have the effect of burying the fundamental issues of human psychology which Freud's drive theory exposes.

II: THE CLASSIFICATION OF INSTINCTUAL DRIVES

In the 1920's Freud abandoned his earlier classification, sexual drives and ego drives, and, at first tentatively, then with increasing conviction, postulated life and death drives as manifestations of omnicellular, biological forces. He never accepted aggression except as a possible derivative of the death drive, as Brenner has pointed out (e.g., 1971b). Freud maintained the derivative nature of aggression, at least in part, because of the lack of an evident somatic source for aggression and the lack of a clear relationship to the pleasure principle.

Freud's postulation of life and death drives, the latter based on the idea of a repetition compulsion, caused considerable debate and controversy in the 1930's and 1940's (see Loewenstein [1940] for a carefully considered discussion). Although Freud's life and death drives were never generally accepted by analysts, no sound theoretical statement upon which to base an independent aggressive drive existed until 1949. Hartmann, Kris, and Loewenstein then worked out the necessary assumptions and their consequences. They were willing to forgo a discernible *source* for aggression, basing the concept on psycho-

analytic clinical (psychological) data. They assumed that aggressive discharge of itself may be experienced as pleasurable, that is, be regulated by the pleasure principle, and that, with respect to *impetus*, libido and aggression might be held to operate in strictly parallel fashion. Libidinal *aims* were seen as varied and plastic; the aims of aggression were taken to be more rigid, but the concept of the aims for aggression was recognized as far less clear. They proposed that both the aim and the *object* characteristics of aggression require a somewhat different discussion than do the corresponding aspects of libido, discussion in terms of modification of aims and modification of impact upon the object. Aggressive trends, they proposed, may be sublimated or aim-inhibited, as are sexual trends. Developmentally, aggression was seen to follow the patterns of libidinal discharge modes—that is, the sequence of libidinal organizations postulated by Freud might be taken as (libidinal/aggressive) drive organizations.

Despite the widespread influence of the paper by Hartmann, Kris, and Loewenstein, most issues regarding aggression as an instinctual drive seem to remain unresolved, and analysts remain widely divided on the issues of aggression. Stein (Panel, 1972) suggested that four groups may be seen: (1) those who follow Freud's formulation of the death instinct (Eissler, 1971; M. Klein, 1958; Rosenfeld, 1971); (2) those who consider that aggression and sexuality may be viewed in a parallel way, including an underlying aggressive energy comparable to libido (Brenner, 1971a; Hartmann, Kris, and Loewenstein, 1949); (3) those who reject the concept of aggression as an instinctual drive, seeing aggression as a result of frustration and nonspecific tension reduction (Fenichel, 1945; Gillespie, 1971; Stone, 1971); (4) those who would prefer to dispense with drive models entirely.

The issues here are obviously broader and more complex than those of the concept of aggression itself. Our concept of instinctual drives determines what phenomena we will try to explain by drive constructs. A problem of drive constructs is

that they can readily explain too much: a different drive can be—and has been—postulated as an explanation for every conceivable type of behavior. Circumscription of the number of drives then becomes essential if the concept is to be useful or even meaningful. Such circumscription is achieved, as in other sciences, by classifying behavior according to features which are then explained by corresponding classes of drives. The classes of drives selected determine the universe of discourse for the instinctual drive explanations. Questions may then be raised concerning the concept, the method or basis of classification, or the content of the classificatory scheme. Scrutiny of any of these areas may lead to a conclusion that things we have called the same are not the same after all, or that they are the same, but not what we have called them.

Such discussion tends to be determined by the content of the current classificatory scheme, and the historical dimension becomes important. Historically, in psychoanalysis there has been a sequence of drive classifications, as noted above, always consisting of two groups. When the universe of discourse contains only two classes, questions about one of the members are inevitably questions also about the scheme of classification and the drive concept itself.

The question of how we decide how many drives there are resolves into two other questions. *How do we decide whether or not a given phenomenon is to be partially explained by a drive construct?* And, *how do we decide whether a given phenomenon, or its explanation, is to be subsumed under a particular drive construct rather than another one?*

There have been a number of suggestions for other classifications of drives or for adding instincts in the biological sense, mostly in relation to the explanation of anxiety. Brunswick (1954), for example, proposed the addition of defensive drives to libidinal and aggressive, and a reclassification into vital-libidinal and defensive-aggressive types. Schur in 1958 proposed an instinct (not drive) to avoid danger, present in animals and men, an idea which he later (1966) extended. He saw the bio-

logical withdrawal response as the model for the anxiety response and for defense (pp. 151-152). In Schur's formulation also the instinctual drives, libidinal and aggressive, are regulated by the pleasure principle, while the instinct to avoid danger is not. The unpleasure principle is related to the avoidance of pain or danger as an instinctive reaction in itself. Rangell (1955, pp. 403-404; 1968, pp. 398-399) and Stewart (1967) also suggested instinctive factors in the production of anxiety. So, too, did Freud, in the idea of "key neurones," which appeared in the *Project* (1895b) but never afterward. These elements of the nervous system were proposed to generate unpleasure, that is, were a source of negative impetus parallel to what he later called libido in the sphere of sexuality and pleasure.

These, or other, alternative views of drives or instincts may have more merit than their lack of acceptance to date would indicate.

Within the framework of the libidinal/aggressive classification, most of the current problems have to do with aggression. The following issues arise concerning the concept of aggression as an instinctual drive. What are the observational data upon which we base the concept of an aggressive drive? What is the relationship between these data and the explanatory construct? To what degree and in what respects is the concept of aggression as an instinctual drive different from or parallel to the concept of libidinal drives? The issue of parallelism to libido raises questions concerning the applicability of the characterizing aspects of source, impetus, aim, and object to the concept of an aggressive drive seen as an explanatory construct. Alternatively, we might see the issue as one of altering the concept of instinctual drives to accommodate aggression as we understand it.

For the psychoanalytic drive model, questions of source and relevance of observational data are closely linked. For the discussion of aggression and source, the earlier statement of this relationship requires expansion.

There are various bodily events which may come to awareness more or less readily, via the activity of afferent (sensory) nerves and their central nervous system pathways. The stimuli which originate activation of the afferent nerve impulses may commence outside the body or within the body. These sensory inputs to the CNS would include the special senses and the general senses. Each of these sensory modalities can be related to specific end-organs, specific nerve fibers, and well-traced pathways through the spinal cord and brain stem. Most of these modalities are known by the names of the conscious sensations associated with stimulation of the end-organs.

In addition, there are bodily sensations of more complex composition which may involve, besides end-organs and sensory nerves, special centers in the CNS. These include hunger, thirst, respiratory need, urge to yawn, urge to sneeze, urge to micturate or defecate, sexual sensations, and perhaps others.

Thus far there is nothing that requires any designation more complex than sensation and awareness of it. All of the complex bodily sensations may increase in intensity to the point of displeasure or distress, and the removal of what is causing the sensation is accompanied by some sense of relief or comfort or pleasure. The simpler bodily sensations may also be associated with feelings of pleasure or displeasure. Awareness of distress and pleasure as accompaniments of bodily sensations requires a more complex neural apparatus and for the first time requires some psychological model providing for more than sensory perception. The activity may then be repeated for its own sake—that is, for the attendant sensations of pleasure.

It seems that recognition of this set of relationships was implicit in Freud's early models of sexuality and probably formed one basis of his concepts of sexual drives and ego drives. Activity is initiated by somatic changes and is terminated by somatic changes.

Developmentally, hunger, thirst, and certain sexual or sensual sensations require a partner; respiratory need, urges to yawn,

sneeze, micturate, defecate, and certain other sexual sensations do not.

None of these considerations of sensations of somatic tension/relief and subjective distress/pleasure apply to "aggression."

None of this—or very little of it—is psychoanalytic clinical data. It is somewhat imprecise neurology and does not depend upon the application of the particular method of psychoanalysis. Bodily sensations may be reported in analytic sessions. Bodily sensations that we would relate to aggression are generally perceptions of somatic changes in the fight/flight sequence—physiologic preparations for action.

If we start with psychoanalytic observational data and ask the same question that we did earlier—what is the relation between phenomena and the concept, this time, of aggression as a drive?—we can pursue the same line of thought, from inference of conscious awareness, data of conscious experience, inference of unconscious mental process and content with the characteristic of driven-ness. We cannot, however, give any answer concerning the impetus of aggressive trends in somatic terms.

In the case of sexuality the eventual criterion for the sexuality of a (mental or physical) behavior is the subjective awareness of a sexual feeling or sensation. Since the method of psychoanalysis is not introspection, but rather that of the psychoanalytic situation, this criterion can only be fulfilled via reports from verbal and willing subjects. For direct observation other criteria are necessary. Typical genital changes are adequate. Rhythmic manipulations of genitals or other body parts might or might not be acceptable as criteria (see Galenson [1971], discussed below).

In analysis we habitually make use of other, less unequivocal criteria. If a given behavior is not experienced as sexual by the reporting subject, but can reasonably be inferred to stem from some underlying impulse or fantasy which is the basis of known sexual behavior in children or perverts, we are willing to interpret the sexuality of the behavior and judge from the results whether or not the inference is correct.

In the case of aggression, the entire situation is less clear. Ideas and feelings involving anger, hatred, hostility, destructiveness, envy, spite, vengeance, cruelty, and so on seem to be warded off in the same way that sexual impulses are; they may be inferred, interpreted, and the effects judged just as is the case with sexual ideas and feelings. This is the psychoanalytic clinical evidence which Brenner, for example, sees as sufficient to justify the assumption of aggression as a drive. Aggressive elements (ideas, feelings, impulses) seem to be mentally similar to sexual elements: they are warded off and are a source of mental conflict.

We thus arrive at the conclusion that the psychoanalytic clinical evidence for aggression as a drive is, in this respect, very similar to the evidence for sexual drives, while the background of supporting, physiologic, and directly observable evidence present for sexuality is totally absent for aggression. In fact, the bodily changes associated with aggressive behavior are those accompanying the fight/flight series of reactions. The somatic referents for aggression seem to occur secondarily, as responses to environmental stimuli, rather than primarily as somatic stimuli leading to the search for an appropriate object (environment), and physiologically are of the general nature of preparations for action, unlike the specific changes seen in sexual behavior.

One of the significant sources of confusion about aggression is the failure to differentiate aggressive behavior, actual or mental, from aggression as an instinctual drive and from aggression as the hypothetical energy behind the drive. If this distinction is carefully made, one may, for example, raise the question: do all behaviors that are descriptively aggressive constitute expressions of an aggressive drive? A commonly held view is that the aggressive drive is intended to account for activity generally. While such a view might or might not be justifiable, it is in discord with the psychoanalytic theory of drives. The concept of drive in and of itself implies impulsion to activity.

There does seem to be widespread agreement that phenomena which may readily be called aggressive are important in human

behavior generally and in clinical situations, whether or not aggression is accepted as an instinctual drive. Marcovitz (Panel, 1979) suggested a "phenomenological classification of interpersonal behaviors that may be labeled aggressive": actions that are not hurtful so long as they are accepted without challenge (self-assertion, dominance, exploitation); hostility, which involves injury or destruction (removal of an obstruction to a goal, panic, defense); hatred, a distinctively human trait; sadism, the infliction of pain to provide sexual excitement (p. 656).

Whether or not Marcovitz's particular classification is accepted, some such phenomenological grouping would seem to be a necessary step (E. O. Wilson [1975], for example, offers a very different classificatory scheme). Phenomena observed in the psychoanalytic clinical situation might more fruitfully serve as a basis. In either event one must ask: what is the tacit basis of the classification?

Review of the observational data upon which we base the concept of aggression and of the relation of these data to the construct demonstrates that aggression is different from and not parallel to the libidinal drive concept so far as *source* is concerned. What about parallelism in terms of impetus, aim, and object?

Drive impetus is not much discussed under the term impetus. Yet the motivational impulsion to activity, reflected in the subjective sense of tension and relief, is the very essence of the drive concept. A. Freud (1972, p. 164) cited the "unmistakable *impetus* which is inherent in any aggressive striving; the unmistakable *relief* which follows its discharge; the unmistakable *distress* and its pathological consequences when discharge is blocked." We shall return to this aspect of aggression when we take up the relation of drives to the regulatory principles. The concept of aggression does seem to fit with the sexuality derived model of instinctual drives in respect to impetus; here aggression is parallel to libido.

What of the *aims* and *objects* of aggression? Anna Freud, viewing the problem developmentally, questioned the widely

held idea that "the aggressive drive itself . . . undergoes consecutive qualitative transformations" in parallel to oral, anal, and phallic sexual organizations (1972, p. 164). She noted major differences between sexuality and aggression in regard to both aims and objects:

> The libidinal aims, whether biologically prescribed, whether direct or sublimated are always specific to the drive. In contrast, aggression can associate itself with aims and purposes of extraneous kinds, lending them their force. We are familiar with this, of course, from the study of infantile sexuality. . . . Aggression also comes to the aid, either constructively or destructively, of purposes such as . . . vengeance, war, honour, mercy, mastery . . . i.e., in the service of aims which are dictated either by the ego or superego (p. 165).

Brenner (1971a) apparently agreed that aggressive aims cannot be treated in strict parallel to sexual aims. "Aggressive aims vary with mental development and experience. They seem to be related to what hurts or frightens a child" (p. 141).

A. Freud (1972) also pointed out changes, developmentally, in the instrumentality of aggression: teeth, excrement, the penis; the voice; the mouth for spitting; hands, arms, legs—the musculature generally; tools—knives, sticks, guns, bombs, poisons (p. 167). In relation to Hartmann, Kris, and Loewenstein's suggestion that the aims of aggression are more limited and rigid than those of sexuality, we might say that the instrumentality of aggression is more varied, plastic, or expansive.

Can the concept of *drive object* be maintained in the same way for aggression as for sexuality? (I shall consider the general question of the relation of drives and objects in the article that follows this one, pp. 414-426.)

In early infancy the development of sexuality and of aggression appears to be parallel with respect to objects, although our data are not particularly reliable for these stages. Spitz (1953) emphasized the role of aggression in establishing object relatedness. A. Freud (1972) observed that the developmental lines of sex and aggression become significantly different after

infancy. Hate, anger, resentment, and related feelings are more closely tied to satisfaction and frustration experiences than to a particular object: "there is no object constancy for aggression as there is for the libido" (p. 165).

Perhaps we can go further in specifying differences in libido and aggression with respect to objects. Freud's early idea of drive included a "specific action" which had to occur in the environment (object) to release drive tension. In this sense there is not even object specificity for aggression, with the possible exception that answering rage, like answering desire in sexuality, may be viewed as an object-specific drive tension releasing factor.

Besides divergencies between the concepts of libido and aggression in terms of drive source, aims, and objects, there are also differences with respect to defensive operations. Denial, repression, reaction formation, projection, identification, turning from object to self, and turning passive into active seem to be equally effective in relation to either drive. Identification with the aggressor, displacement of object from human to inanimate or animal, and undoing in obsessional neurosis seem to be directed against aggression only (A. Freud, 1972, p. 166).

The controversy which centered around the idea of life and death instincts has had unfortunate results: after sixty years clinical study of relevant phenomena and the formulation of clinical hypotheses about aggression are still in their infancy. Parallelism between aggression and sexuality is a temptingly convenient notion, but one which not only is not established but also is clearly impossible in many respects. Aggression requires at least somewhat different concepts, and it may be that the psychoanalytic concept of instinctual drive, worked out almost entirely on the basis of data on sexuality, has constrained observations on aggression, leading rather to unproductive theoretical disputes. Careful clinical studies, generating hypotheses with a minimum of intrusion by sweeping abstractions are still rare (see, however, Parens, 1979a). The idea of sexuality as an innate, somatically determined mental force

with tension buildup and discharge/pleasure modes is generally applicable to all phases of development. Ostensibly parallel ideas on aggression involve a cognitive appreciation or inherent awareness of the effects of aggressive actions upon objects, concepts clearly not applicable in a general sense (see Brenner, 1982).

When careful studies are more abundant, perhaps we shall choose to view aggression as something other than a drive. Perhaps we shall want to modify the drive concept, as Brenner does, to accommodate what we know about aggression. There are a number of other possibilities. Acceptance of the libidinal/aggressive universe of discourse has tended to obscure certain difficulties. The discrepancies between the concept of libidinal drives and the concept of aggressive drives are often disregarded. Distinction of the drives from the hypothetical energies behind them is often blurred or absent. A false impression of two qualitative forces with parallel vicissitudes tends to be created. Perhaps aggression must be viewed as a different kind of drive, which means that the psychoanalytic theory of drives requires modification to encompass aggression.

III: THE DEVELOPMENT OF DRIVES

Psychoanalysis has always been a developmental psychology, not only in the sense of studying and describing development, but also of seeing development in a genetic sense, as a cause, origin, or force determining, limiting, and generating later trends (Hartmann and Kris, 1945). Analytic theory has followed the device which seems to be standard in developmental or historical approaches, that of dividing a process continuous over time into phases or stages or periods. The phases are selected by the observer, usually or always with two objectives: differences over time in the selected developmental variable are highlighted; and the time variable, the chronology, is subordinated to the developmental variable, that is, the phase does not necessarily have to occur at precisely the same age in every individual.

Issues arise in a developmental or genetic approach in the following areas: (1) the application of the phase concept—how does one decide what a phase is to consist of?; (2) the sequence of phases; (3) the content of each phase; and (4) the relation of the selected variable to other aspects of, in this case, personality or behavior for each phase.

In considering the development of drives, we are continuing to deal with an inferred variable, that is, a theoretical construct, now with one additional level of complexity introduced by the developmental perspective, and a second by the postulate that psychological regression as well as progression may occur. Phenomena partially explainable by the drive construct are compared at different ages, isolating the drive construct as necessary for such comparisons. A methodologic difficulty also arises, in that direct observation of children, as opposed to the psychoanalytic method, necessarily plays a greater role. Problems of integration of propositions arise because of the different method and the different observational base.

The issues of the theory of instinctual drive development range from philosophical to methodologic to political. How, again, do we account for the ontogenetic origin of mind? Psychoanalytic clinical data on very early stages of development are not reliable. We must turn to direct observation of infants and children to make or confirm hypotheses about drives in these phases. How do we integrate hypotheses derived from direct observation with those derived by use of the psychoanalytic method? This issue, especially, focuses the question of criteria for both direct observation and clinical psychoanalytic data. Issues concerning drive development in girls and sexual function in adult women have also remained controversial. Has recent work, clinical or by direct observation, brought about changes in the theory of drive development in females?

Freud postulated a sequence of developmental phases of the sexual drives, each named according to a predominating erotogenic zone and a mode of object directedness: oral-cannibalistic,

sadistic-anal, phallic-oedipal, and, after an intervening period of relative drive latency, genital. In each of these developmental phases the scopophilic/exhibitionistic and sadistic/masochistic component drives are apparently organized according to the predominant zone and mode (see Compton, 1981a, 1981b). Each of the developmental phases also includes bisexual trends, but these are not clearly distinguishable in Freud's work until the phallic-oedipal phase in which the positive (heterosexual) and negative (homosexual) oedipal attitudes are recognized. The fate of what Freud called "homosexual libido" in the genital stage was less clear but was related to the formation of the "ego ideal."

While the basic elements of this sequence of sexual drive organizations were not significantly altered in Freud's later work, after the introduction of the ego-id-superego model drive development overall began to be viewed somewhat differently, in relation to the structural differentiation of the mental apparatus. This was reflected in the concept of the undifferentiated phase, introduced by Freud in 1938 (see Freud, 1940) and developed by Hartmann (1939) and Jacobson (1954, 1964) (see Compton, 1983, pp. 402-408). Hypothetically, once ego-id differentiation has commenced, the notion of drive organization begins to apply, and the developmental sequence of drive organizations is an aspect of id development. (Recall once again that these terms all designate theoretical constructs, such that phenomena are to be explained in terms of the interaction of the constructs.)

These ideas about steps from an undifferentiated phase to the beginnings of mental structure and mental forces in the form of drives relate to "getting the mind started" developmentally, in a highly abstract fashion. In an attempt to come closer to phenomena, Schur (1966), following his earlier ideas on desomatization (1955), proposed that the mental apparatus of the newborn does function according to the model of a reflex apparatus. A transition or progression then occurs, from reflex to wish. This is the model for the transition from somatic

needs to instinctual drives as mental representations of stimuli of somatic origin. This transition also marks the development of the structure id from the undifferentiated phase (and presumably of the earliest form of the ego as well).

Although methodologic and conceptual problems make the integration of observations with these hypotheses hazardous, Dowling's (1977) work with infants afflicted with esophageal atresia apparently presented observations relevant to the developmental sequence that Schur postulated. In these infants the gastrostomy separates nutritive need from oral experience. In one case particularly (pp. 230-233), development from reflex sucking and swallowing to purposeful, motivated oral demand could be seen very clearly.

The developmental differentiation of psychic structure and the concomitant and ensuing developmental sequence of libidinal organizations are usually seen as the result of a genically encoded program (Brenner, 1955, p. 17). Developmental changes in drive organization are explained at a psychological level as a detachment of libido from an object and/or mode of gratification, and cathexis of a new object and/or mode (p. 28). The work of Dowling and others clearly indicates that the unfolding of the genic program depends upon the opportunity for zonal interaction with the environment. In terms of classical drive theory, the availability of an erotogenic zone and of a suitable object partially determines drive development.

Since the 1949 publication by Hartmann, Kris, and Loewenstein, which proposed some degree of parallelism between libidinal and aggressive drives, Freud's sequence of libidinal organizations has generally been taken as a sequence of drive organizations, with all of the component drives having aggressive as well as libidinal aspects. Propositions specifically concerning the mechanisms of advance of aggression are, however, far less clear or wanting altogether. I noted above some questions concerning the parallelism and the kinds of data offered in support of these hypotheses.

Freud's proposals on drive development have been modified

in a number of other significant ways. Many of these modifications also derive from the direct observation of children. Mahler (1966) mentioned the child's discovery of anatomical sexual differences at a much earlier age than had been previously recognized—fourteen to sixteen months. Roiphe and Galenson have been pursuing this suggestion in a series of ongoing studies, amply confirming and expanding the observation (Galenson, 1971; Galenson and Roiphe, 1971, 1976, 1980; Roiphe, 1968). They proposed an "early genital phase" as a normative developmental event, starting between sixteen and nineteen months, during which "the genital zone emerges as a distinct and differentiated source of endogenous pleasure. . ." (1976, p. 54). This early genital phase is associated with discovery of anatomical sexual differences and a "preoedipal castration reaction," which becomes fused with earlier fears of both object and anal loss and also influences the later phallic-oedipal phase as well as other aspects of development. Obviously, boys and girls have different reactions to this early genital phase and show significant differences in subsequent psychological development.

These studies are pertinent to proposals for modification of Freud's hypotheses on female sexual development. We have seen that Freud came to the study of male/female differentiation in childhood only after 1925 (although he was clearly aware of such differences in early infancy in 1905 [pp. 188-189]) and in connection with his interpolation of the phallic-oedipal phase into the developmental sequence (Compton, 1981d). Freud's propositions did not systematically extend the idea of male/female differentiation to younger ages or developmental phases earlier than the phallic-oedipal.

Freud's ideas on female sexual development and function (1925, 1931, 1933) aroused controversy (a not unusual result of Freud's ideas!) from the time of their introduction (Horney, 1923; Jones, 1927; M. Klein, 1928). Phallocentric bias against women is held to invalidate psychoanalytic views of women, then extended to dismiss instinctual drive theory and psycho-

analytic psychology in general. Given the essential ideas that girls, in the course of their development, must change their primary love object from mother to father, must change their leading erotogenic zone from clitoris to vagina, and have a different relation of the castration complex to the oedipus complex than boys do, what are the issues that are raised? First, are girls not different from boys from the beginning? If so, what relation does this have to the instinctual drive development of girls in the preoedipal years? Second, does a change in the "leading zone" from clitoris to vagina actually occur? Is there "early vaginal awareness" and, if so, what effect does this have on the leading zone hypothesis? What bearing do "types of orgasm" in women have on this proposition? Finally, if a change in the primary love object occurs, how does this come about?

These issues must be viewed in the context of the psychoanalytic theory of drive development in general, and especially the theories of genital primacy and of preoedipal development. Freud's 1905 concept of genital primacy was hardly that of orgasm location. The changes from infantile to genital sexuality were held to include: (1) transition from autoerotic to object-directed predominance; (2) transition from separate or loosely organized drives and zones to a thoroughgoing genital taking over—"fitting in"—of all sources of excitation; (3) change from general pleasure as an aim to a specifically sexual, genital aim; and (4) convergence of sexual and affectionate currents on the same sexual object and sexual aim (1905, p. 207).

Similarly, Freud's concept of preoedipal development was not simply another name for the oral and anal phases. It entailed a retrospective view, from the standpoint of the oedipus complex, upon how that set of drive and object orientations came into being out of the background of oral-cannibalistic and sadistic-anal organizations and corresponding phases of ego development.

Differences in the development of boys and girls began to become clearer in relation to the preoedipal view. For boys the question is: of what does the change in attitude toward the

retained preoedipal object (mother) consist? Loewenstein (1935) and Lampl-de Groot (1946) answered this in terms of the boy's turning from passive phallic aims to active phallic aims.

For girls, the issues, as suggested above, extend to earlier developmental phases.

Since the 1920's a number of authors (e.g., Horney, 1923; Jones, 1933; Zilboorg, 1944) have argued the case for "primary femininity" and, in the case of Horney, primary heterosexuality. (It seems fair to say that heterosexuality is seen classically and currently by most analysts as a developmental achievement, a product of mental differentiation and not only of physiology.) Stoller (1968, 1973, 1975, 1976, 1979) has recently discussed the same idea of female sexual development in his concept of "core gender identity," the sense of maleness in men and of femaleness in women (primary femininity), present as a developmental consideration continuously from prenatal life. Stoller proposed that this gender sense depends upon: biologic forces organizing the fetal brain; sex assignment at birth; the mother's attitude toward that sex; the effects of early postnatal handling patterns; the developing body ego, especially defining effects of genital sensations; and mental development.

There seem to be two sets of issues here. The first concerns early developmental phases. As Stoller pointed out, sexuality is not the same as gender sense, and neither is the same as object choice. The construct of instinctual drives is intended as explanatory of, among other things, sexuality and, in part, of object choice. What is the relation of the drive concept to the concept of sense of gender? The pregenital drive organizations would seem to be not much related to early gender sense. Gender sense and instinctual drive would come into relationship only when the drive organization has advanced to a genital —that is, phallic or early genital—stage. On the other hand, early vaginal awareness would indicate unconscious representation as well and would necessitate some modification of Freud's concepts of pregenital organization in girls to include such representation. This might or might not have the effect of

establishing a "feminine developmental line," implying that vicissitudes of drives and their derivatives are different for boys and girls at every developmental phase.

Lack of awareness of the vagina by both sexes in childhood was an important element of Freud's picture of the phallic-oedipal phase. Bonaparte (1935) postulated early vaginal awareness, but attributed it to an overflow of rectal and clitoral sensations in her "cloacal theory." Greenacre (1950, 1953, 1958) presented psychoanalytic clinical evidence for vaginal awareness in girls from early infancy onward. Kestenberg (1956a, 1956b, 1968) also presented evidence of early vaginal awareness and suggested a far-reaching influence of this awareness in maternal behavior and adult female sexual function. Fraiberg (1972) made observations on the same topic and added a collection of cases of masturbatory orgasm, or something close to it, in girls ages four to nine.

Early vaginal awareness with unconscious representation of vaginal tensions, the persistence of masturbation in latency girls, and the presence of prepubertal orgasm-like responses all would seem to require alterations of or additions to Freud's view of female sexual development.

The second set of issues has to do with suggested modifications of later infantile phases of girls' sexual development, not based on the concept of primary femininity or the observation of early vaginal awareness.

Galenson and Roiphe (1976) noted the following divergences in connection with the early genital phase and the child's discovery of anatomical differences. In girls, there is an increased investment of fantasy life, a change in basic mood, an alteration of masturbatory patterns (away from manual), and a turning to the father "in a newly erotic way, seeking the mother's attention only during periods of distress." Boys, in contrast, showed persistence of manual masturbation, increases in general motor activity, and less tendency to fantasy play and graphic representation (pp. 49, 51). They concluded, "We believe that Freud's original position was partially correct in that penis envy and

the feminine castration complex do exert crucial influences upon feminine development. However, these occur earlier than he anticipated . . . and they shape an already developing, although vague sense of femininity" (p. 55).

Grossman and Stewart (1976) seem to be approaching the same developmental considerations from the viewpoint of psychoanalytic clinical study of adult women. They postulated two phases of penis envy. The first occurs as a narcissistic injury, which may be resolvable and may contribute to female development, or become one of a series of envy-producing traumas in a narcissistic character. The second phase is a regressive attempt to resolve oedipal conflicts.

The debate over the change of the girl's leading erotogenic zone seems to have been carried out on false premises. In the first place the data were obfuscated. Glenn and Kaplan (1968) pointed out that "clitoral orgasm" and "vaginal orgasm" appeared in the literature without definition as terms and may mean: (1) the area stimulated to bring about orgasm; (2) the area in which the orgasm is subjectively experienced; (3) the area in which physiologic changes occur in the genitalia; and that a variety of combinations occur. To this it should be added that Freud's theory did not include the terms clitoral or vaginal orgasm, but referred to libidinal cathexis of (the representations of) anatomical regions.

The developmental issue of the phallic-oedipal phase in girls has been summarized in this way: "The issue that needs to be examined is whether or not heterosexual object choice in women is tied to a reaction against 'active aims' in sex" (Grossman, 1981).

Issues regarding the roles of narcissism, masochism, and passivity in female development are less clear. The idea of a primary erotogenic feminine masochism has also been considerably challenged (see Blum, 1976; Brenner, 1959).

The well-known and widely respected work of Margaret Mahler would seem to have bearing upon psychoanalytic theories of drive development. Integration of the two bodies

of hypotheses has, however, proven difficult and seems unsatisfactory in most respects at this time (see, however, Parens, 1979a, 1979b).

In summary, Freud's hypotheses regarding a sequence of developmental phases of instinctual drives remain as the basic psychoanalytic theory of drive development. The earliest phases of drive development are now viewed in relation to the onset of differentiation of psychic structure. Developmental phases now tend to be seen as drive organizations rather than only libidinal organizations, although this formulation is open to serious question. The child's discovery of the anatomical differences between the sexes is now recognized as being significant at a considerably earlier age. Sexual behavior in the latency period has been found to be more common in both boys and girls than Freud thought. Issues of female sexual development remain controversial.

The implications of much of the recent observational work upon the general theory of instinctual drives and drive development remain to be determined.

SUMMARY

The evolution of Freud's theory of instinctual drives, with the accompanying models of a mental apparatus, is remarkable for its tenacious adherence to addressing the fundamental problems of human psychology, here phrased as the problems of body-mind-environment relationships.

The concept of instinctual drives continues to be one of the most pervasive concepts of psychoanalysis, weathering considerable attack over the last several decades, although losing some clarity in the process. I have cited and discussed as basic issues of the concept of instinctual drives: the relationship of observational data and theoretical constructs in psychology; whether our construct of drives is or should be or can be purely psychological; the problem of conceptualizing the ontogenetic origin of mind; the issues of the "force-meaning conjunction" and the

problem of psychic energy in psychoanalytic constructs; and the relation of our concept of instinctual drives to the concept of instincts in general. It seems that progress with these fundamental issues might be made by utilizing models that are more homologous with present knowledge in related fields than is Freud's reflex arc model of the nervous system, in order to build a better drive construct within the framework of psychoanalysis.

The classification of instinctual drives remains a problem. Clinically, aggression seems to be a factor in conflict, very much like sexuality. Despite widespread acceptance of the idea of aggression as simply parallel to sexuality in all respects, there are major discrepancies. Perhaps aggression cannot be viewed as a drive after all; perhaps our drive construct needs to be modified to accommodate aggression. Certainly, controversy in this area has interfered with the production of good clinical studies which could begin to increase our understanding of aggression and its place in the human personality.

The psychoanalytic theory of drive development has probably undergone less change in the last forty years than other aspects of drive theory, although careful observational work has led to alterations of our views of the age of onset of genital awareness, of female sexual development and function, and of the latency period.

This is the conclusion of the first of two papers examining the post-Freud development of the theory of instinctual drives in psychoanalysis. The companion paper follows on p. 402 of this issue of *The Psychoanalytic Quarterly*.

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HELENE DEUTSCH

1884-1982

With the death of each eminent European colleague, we feel again the passing of an earlier, more exciting era in the history of psychoanalysis. But among these illustrious forebears, Helene Deutsch occupied a unique position, because her long life connects us with the most exhilarating, tragic, eventful years of our past. Born Helene Rosenbach in 1884, she grew up in Przmyśl, a Polish-speaking garrison town on the Ukrainian border of the Hapsburg Empire. She was the youngest daughter of a district magistrate and an ambitious, difficult mother. As a girl, she was not admitted to the local *Gymnasium*, which obliged her to prepare for university entrance requirements by other means. She ran away from home several times, studied in Lwow and Zürich, where she met famous members of the revolutionary intelligentsia, and had a romantic affair with a young Polish radical. She was fond of recalling her youthful activities in behalf of workers' and women's rights and her attending the 1910 Congress of the Second International in Stockholm.

In 1907 she was finally admitted to the medical faculty of the University of Vienna, one of seven women in her class, only three of whom completed their studies. In her last year, while studying the word-association test with Kraepelin in Munich, she met Felix Deutsch, a promising young Viennese internist who had come to give a lecture. They were married in 1912 and settled in Vienna where she pursued her training in clinical psychiatry on Wagner-Jauregg's service at the University Hospital. She became interested in psychoanalysis through her own reading and the encouragement of Paul Schilder, and after her son, Martin, was born in 1917, she began a training analysis with Sigmund Freud. She became a member

of the Vienna Psychoanalytic Society in 1918 and published her first clinical paper a year later. In 1923 she felt the need for further analysis, which she obtained, at Freud's suggestion, from Karl Abraham in Berlin. Since 1920 the Berlin Psychoanalytic Institute had been the first analytic society to organize a systematic training sequence of didactic analysis, supervised cases, and three to four years of seminars and practical experience in their outpatient clinic. Helene Deutsch taught a clinical seminar there in the spring of 1923, with her analyst as coteacher. She gave her own seminar the following year, "The Relations between Psychic and Sexual-Physiological Phenomena in Women," foreshadowing her later interest in feminine psychology.

When Helene Deutsch returned to Vienna, she played an active part in establishing a training institute based on her Berlin experience. She introduced the continuous case seminar, which has become a standard feature of analytic training all over the world. She represented Vienna at the 1925 International Congress at Bad Homburg and remained director of training at the Vienna Institute for many years. Among her pupils and younger colleagues she was celebrated as a brilliant teacher and an intuitive clinician of formidable powers. Beginning her seminars at 9:30 in the evening, according to Richard Sterba, she could keep her candidates awake until 2 a.m., spellbound by her penetrating, almost uncanny insights. She was also famous for her sarcasm and acerbic wit.

In 1930 Helene Deutsch was one of eight eminent Europeans to be invited to the International Congress of Mental Hygiene in Washington, D.C., where she was enthusiastically received as a representative of Viennese analysis. Her husband Felix, who had become an analyst in the early 1920's and a pioneer in psychosomatic medicine, came to lecture in the United States during late 1933 or early 1934. Hitler had come to power in Germany in early 1933, and Felix Deutsch found a greater awareness of the Nazi threat to Austria in this country than in Vienna. The Deutsches again considered the possibility of

emigration (Helene had declined Franz Alexander's invitation to the United States in 1929). Robert Waelder, who had foreseen the danger of native Austrian fascism as early as 1927, was the only colleague who encouraged them to leave. The opportunity came in 1935, with an invitation from Professor Stanley Cobb of Boston, for Felix to continue his psychosomatic research in a new department at the Massachusetts General Hospital, the first general hospital psychiatry unit of its kind. Helene and her son left in September 1935, in time for Martin to enter the Massachusetts Institute of Technology in the fall term, and Felix followed three months later. The Deutsches were welcomed by scientific colleagues in both medicine and psychoanalysis, and Helene re-established contact with several of her former analysands from Vienna. The Boston Psychoanalytic Institute had completed its painful reorganization only two years before, and Hanns Sachs, of Vienna and Berlin, was still the only training analyst. After a return visit to the Marienbad Congress in the summer of 1936, the Deutsches made their final decision to settle permanently in Boston. They were well established here before the major migration of refugee analysts that came between 1938 and 1942 and transformed American analysis within the next decade.

Both of the Deutsches worked diligently as training analysts, teachers, and committee chairmen, and each in turn served as president of the Society. Both entered upon a period of new and increased scientific productivity in their adaptation to a new country. For Felix this was his first transplantation, but the second for Helene, who thought of her life as divided into a Polish, a Viennese, and an American period. The creative stimulus was more obvious in Felix's career, because his special field of psychosomatic medicine received a more enthusiastic reception here than in Vienna, but Helene produced some of her most original papers during the late 1930's.

Although she had always been prolific, and her collected writings cover the whole range of neurotic symptomatology, her originality lay in her special interest in the psychoses and

certain unusual borderline states, as revealed in her well known paper on the "as-if" personality. This paper, originally published in 1934, owed something to Chekhov's story, "The Darling," she has said, and illustrated her ability to connect clinical observations with literary prototypes, as in "Don Quixote and Don Quixotism" (1937) and in her magnificent study, "The Impostor" (1955). When her papers on mania and depression were written, relatively few analysts were interested in the psychoses, and her keen observations on "absence of grief" and the emotional reactions to surgery were also highly original for their time.

Helene Deutsch's contributions to the understanding of borderline personality disorders have been acknowledged as the forerunners of the current wave of interest in these problems. But her monumental work on the psychology of women, for which she was best known to the general public, has undergone a recent eclipse in popularity. Nevertheless, her two-volume study still represents the only coherent developmental approach, from the early years to motherhood to the climacterium. From the beginnings of the women's liberation movement, Helene Deutsch has been both hailed for her pioneering work in women's studies and attacked for her acceptance of Freud's views of female sexuality, especially her defense of masochistic passivity as a "normal" developmental phase.

Although both Freud's and Helene Deutsch's views are more complex than these stereotypes, there are important theoretical issues to be re-examined, and Dr. Deutsch had largely ignored both her popular and her analytically sophisticated critics. Instead, during the last decades of her retirement she turned to topics that interested her more, beginning with a collection of interviews with "normal adolescents." These were non-patients who found an unexpected warmth and understanding in a woman their grandmothers' age, and her observations were published as a monograph in 1967. Two years later she delivered the Freud Anniversary Lecture, a study of the myths of Dionysus and Apollo, "two variants of the mother-son rela-

tionship." A modest contribution to the 1970 Jerusalem Symposium, "Why War?," followed a discussion of Dr. L. Rosenberger's paper, "The Woman's Role in Aggression." This was an unabashed paean to the struggles for peace of Rosa Luxemborg, Angelica Balabanoff, and the Baroness von Suttner, reflecting her own early revolutionary interests and her continuing sympathies with the youth movements and antiwar activities of the late 1960's. Her last publication was a lively memoir (1973), leaving us with an engaging picture of herself as she wished to be remembered.

During the last decade of her long life, she remained in remarkably good physical health, despite two cancer operations, cataracts, and osteoarthritis. She continued to read, to keep in touch with current events, and to entertain her younger friends with amusing recollections. She was painfully aware of her progressive memory loss and found ingenious methods of dealing with its limitations. She never lost her unusual ability to make new relationships, her sarcastic wit, and her affectionate ties with a small circle of family and old friends. Her death on March 29, 1982, at the age of ninety-seven could rightly be regarded as due to "old age," when her memory no longer supplied sufficient gratification from recollections of her long and eventful past.

SANFORD GIFFORD

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Becoming a Psychoanalyst. A Study of Psychoanalytic Supervision. Edited by Robert S. Wallerstein, M.D. New York: International Universities Press, Inc., 1981. 351 pp.

David M. Sachs

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BOOK REVIEWS

BECOMING A PSYCHOANALYST. A STUDY OF PSYCHOANALYTIC SUPERVISION. Edited by Robert S. Wallerstein, M.D. New York: International Universities Press, Inc., 1981. 351 pp.

In the early nineteen-sixties, psychoanalytic educators were concerned that the methods and goals of supervisors might have been more diverse than those of training analysts and of faculty members teaching didactic courses. If their concerns were justified, it indicated that an imbalance existed in the tripartite model of psychoanalytic education that would make it difficult to maintain educational standards. The Committee on Psychoanalytic Education (COPE) of the American Psychoanalytic Association decided to investigate matters by appointing a Study Group on Supervision, in 1965, to examine the educational, philosophical, and psychological assumptions that comprise the foundation of supervision. To fulfill all these ambitions would require more than one project, but the Study Group accepted the challenge. This volume is a fascinating documentation of the way in which they discharged their assignment.

The Study Group began by reviewing letters from supervisors describing how effectively they believed candidates "mastered the clinical practice of psychoanalysis" (p. 2). But this approach showed them more about how supervisors evaluate what candidates learn than about how supervision works as an educational process. Since it was the latter issue that the Study Group wished to examine in detail, they designed a research project to elicit the desired data. One member of the group, Herbert Schlesinger, prepared process notes on his first fourteen supervisory sessions with a candidate-analyst. Neither the candidate-analyst nor his patient were informed that the study was being done, since the Study Group hoped to have data uninfluenced by reactions to involvement in a study. Two unexpected consequences resulted from this. First, there is evidence throughout the book that, despite the lack of feedback from the Study Group, being in a study influenced the supervisor anyway, and the effects were transmitted to the candidate-analyst. Second, the failure to obtain consent from those studied stirred

complex legal problems which had to be untangled before the book could be published. It was this latter factor that accounts for the long period, sixteen years, that elapsed between the Study Group's inception and the publication of its findings. That a secret study of confidential material can significantly influence analytic and supervisory processes is at once the least expected and the most illuminating contribution of this study to psychoanalytic research.

This book, then, bears the scars of a long and troubled pregnancy and birth. The problems that beset its passage from wish to fulfillment parallel similar problems that affected psychoanalysis in the United States as it moved from its relatively untroubled childhood in the post-World War II period to its present status as an organizational adult in a culture that does not altogether welcome it. When psychoanalysis was in its organizational infancy, the legal system adopted a laissez-faire attitude toward educational practices that infringed upon an individual's constitutional rights. A stormy period of organizational adolescence occurred between 1965 and 1980, during which massive upheavals in this area of the law began to affect educational practices everywhere. To the present-day reader there is a curious, anachronistic quality to the voices of the nine contributors as they cross and recross the data of the research project in blissful innocence of their violation of the right to informed consent of those who were being studied secretly. Although no one would neglect this issue today, it is well to remind ourselves that the Study Group acted in accordance with the traditions of the time. They believed that they were protecting, not harming, those studied, and that they were preserving confidentiality in accordance with their understanding of professional ethics.

Two vortices of great complexity swirl around the effects of the research design upon the discussions in the book. The first is an ethical issue. Is it possible to justify a secret study of confidential material on the ground that the results will be of greater value than the dangers to the individuals whose rights are being invaded? In short, does the end justify the means? This question deserves serious debate among psychoanalysts, so that the profession can set ethical standards. The second vortex revolves around the effect of secrecy on the meaning of the term "data" in the study. They surely are not the kind of data produced by the analytic process. Since they are "data" of another kind, what happens when they are treated

as though they were analytic data? How can hypotheses generated by such "data" be disproved? The study could be seen as analogous to an in vitro experiment in medicine, as distinguished from an "in vivo" experiment involving actual supervision. In medicine both approaches have made valuable contributions to scientific knowledge, but only when the distinctions between them have been carefully maintained, so as to prevent misapplications. To the great credit of the Study Group, these topics are made available for discussion in the book through the presentation of different versions of what happened. A brief overview of the organizational plan of the book will be helpful in understanding how this is done.

Wallerstein opens by summarizing the chapters. This provides a useful guide to the reader of what is to come. Daryl DeBell follows with an in-depth discussion of the range of methods available for studying supervision. He states in brief terms the psychological and philosophical assumptions of these options. The goals of this chapter are so ambitious that, despite the lucid analysis, the dizzying array of ideas is hard to absorb. He is easier to follow when he gives a description of the San Francisco and Fleming-Benedict models for the study of supervision.

The next chapter, Schlesinger's, describes areas of solid agreement among members of the Study Group about supervision. For example, they all agreed that supervision is an "enabling process" that is "to facilitate [the] process of combined personal and professional growth in the candidate" (p. 30). Teaching is intended to overcome "dumb spots," while some form of psychotherapy is needed to overcome "blind spots." These goals have great appeal, but how to know when to teach and when to treat are everyday thorns in the sides of supervisors. Regarding supervision as an "enabling process" does not provide a means for the supervisor to distinguish the "dumb" from the "blind." As Schlesinger writes:

> None of us was trained to be a supervisor. In comparison with the care given to our education as analysts, our elevation to supervisors was largely a function of seniority and the impression gained by others of our competence as analysts, and perhaps as seminar leaders. For the most part, then, each member of the group was self-taught as a supervisor, and showed it at the beginning of the study in the individuality, even the idiosyncrasy of his working style, in his particular pattern of insights and oversights, and in his conviction that he knew the way in which supervision should be done (p. 38).

This suggests that most supervisors who have not had an intensive experience over a long period of time discussing with a group of peers what they do when they do supervision are following, in the main, idiosyncratic methods that each has devised for himself. Although everyone might agree with the Study Group that his method is an "enabling process," it is evident that this term actually refers to an after-the-fact assessment of what each hopes to accomplish. To describe the methodology of supervision adequately, it would be necessary to describe what the "enabling process" is and how it "enables" the supervisor to determine when to teach and when to treat.

In Chapter 4, DeBell describes some of the dilemmas confronting a supervisor. For example, how much or how little should he utilize his experience as an analyst to educate? How much or how little should he attempt to undo some "blind spots" by doing some form of psychotherapy? And so forth. Agreement exists among the Study Group members that all supervisors face these dilemmas and that they answer them in accordance with assumptions about how to do supervision which are as diverse as COPE suspected they might be. The edifice of psychoanalytic education resembles the Leaning Tower of Pisa, and the foundation of supervision begins to crumble when it is examined. These first four chapters form a natural unit of the book, in which the general educational, philosophical, and psychological problems of the supervisory process are discussed in order to prepare the reader to consider the research project from an informed point of view.

We are now ready to examine Schlesinger's process notes on the supervision. It must have been a difficult task to discipline himself to record these fourteen sessions so faithfully. Not only does he succeed but, in addition, he describes them in vivid language that conveys his impression of the experience. Joan Fleming, one of the originators of the Fleming-Benedict model of supervision, applies it to interpret the "data." As Howard Shevrin points out later, the model was not intended to handle data obtained in this way, but was intended for the processing of tape recordings. However, Fleming does not object to applying the method to the far more limited sample which process notes represent. Her observations about the interactions between the supervisor and the candidate-analyst are subtle, perceptive, and most impressive demonstrations

of how a seasoned and brilliant analyst can find new ways of understanding such interactions. Other contributors, Emanuel Windholz, Peyton Jacob, and Rudolf Ekstein, who discuss clinical details as part of their chapters, also demonstrate a high level of clinical acumen deriving from years of experience and a profound understanding of the psychoanalytic process. Even if one decides that the data they use are inadequate, in the sense that they do not accurately represent what went on in the actual analysis, much can be learned from reading and rereading the way in which these analysts think about the material.

In Chapter 7, Wallerstein carries out a tour de force, in which he reorganizes each session of Schlesinger's process notes in accordance with the various categories of the San Francisco model: the analysis, the analyst's understanding of the analytic material, the analyst's activity, the supervisor's understanding of the technical analytic issues, the supervisor's pedagogic interventions, the supervisor's understanding of the analyst's personal involvement, the supervisor's therapeutic interventions, and the analyst's response to the supervisory hour. He tracks the data in each category through the fourteen supervisory sessions, creating an entirely different impression of the process.

Windholz, in the next chapter, discusses the implications of the reorganized data and notes a sharp increase in didactic teaching that began in the ninth supervisory hour. This is correlated with the observations made in the eighth hour that the analyst was experiencing a strong countertransference reaction. This would have been difficult to appreciate if one had relied only upon the original data without subjecting them to the organizational schema. Windholz enthusiastically endorses the San Francisco model for use in studying the supervisory process because it "reflects comprehensively the events occurring among patient, analyst, and supervisor. It is an indispensable guide for an intensive study of these correlations in the original process notes" (p. 169). He devotes the bulk of his chapter to an "illustrative exposition" of the correlations uncovered by this method of organizing the process notes.

When Jacob continues to examine the application of the San Francisco model, he reaches the same conclusion about its value. His emphasis, however, is different. He states that he "tried to discern the frames of reference and the guiding principles that deter-

mined the way this particular analyst went about his work" (p. 209). Because he deliberately takes a stance which allows him to consider the analyst's technique as a product of his frames of reference rather than as expressions of either "blind or dumb" spots, he hopes to "open our eyes to conceptual frameworks, guiding principles, and new techniques we may be blind to because of rigidities and dogmatic adherence to our own operational 'set' or principles" (p. 209).

Next, Ekstein approaches the data by seeking to free himself from any preconceived model by which to analyze it. He sets himself the task of thinking about the material as though he were the supervisor's supervisor. In the process, he demonstrates a finely tuned empathic capacity. As he debates the options of what to say and what not to say, he reveals his own mental processes and makes clear his reasons for following the path he suggests. He describes working with the data provided by Schlesinger as doing "data research" (in vitro), and he admits to being troubled by his inability to test his hypotheses in the cauldron of the actual supervisory experience. In an actual supervision he would consider what he is doing to be "action research" (in vivo). Ekstein's achievement consists in converting Schlesinger's data into action research by placing it into a frame of reference in which he tests his own hypotheses by imagining various possible results and selecting the interventions he imagines would be best.

Shevrin, the candidate-analyst, makes his contribution in Chapter 11. The placement of his contribution at this point in the book creates a silent parallel with the original study, in that Shevrin found out about the research project only after much of the work had been completed. His remarks portray both the shock he felt when the study was presented to him as a *fait accompli* and the shock the Study Group must have felt at his outrage and disappointment in them. With admirable frankness he tells the story of how he reconciled himself to what had happened, and in the telling he reveals surprising new vistas on the data. He introduces excerpts from his own process notes on the material that Schlesinger has covered. He also discusses aspects of his personal relationship with Schlesinger and of the educational setting of supervision in the context of an institute in turmoil. A radical shift in the meaning of the data presented by Schlesinger occurs to the reader after

Shevrin's data are reviewed. For example, our understanding of the analyst's style, his understanding of the patient, and the location of his "blind and dumb spots" are altered. In addition, Shevrin makes original and valuable observations about analytic process, transference, and the problems of small institutes.

Wallerstein (Chapter 1) had earlier presented the Study Group's view of Shevrin's contribution by describing it as "a vital corrective and modification of the total tableau offered by the supervisor and the other presenters, a proper rounding out of the overall effect" (p. 15). But it could equally well be argued that Shevrin pulls the rug out from under the study itself by challenging the data base on which it rests. In essence, his argument is that the omission of his observations makes the "data" inapplicable to his supervision and to the analysis that was supervised. Wallerstein believes that Shevrin's contribution is additive rather than injurious, creating a *Rashomon* effect in that, since there is no way to choose among the different versions of the story told, all versions are equally plausible. All readers can decide how they are affected by what Shevrin says and how this will affect their views of the study.

The remainder of the monograph consists of three chapters in which the reader has another chance to experience the *Rashomon* effect of Shevrin's contribution. Paul Myerson is convinced that group examination of what one does in supervision has great value. He knows that it changed him in many ways, but he notes that although changes occurred among everyone, these changes did not create a new, shared, uniform approach to supervision. He restores the Leaning Tower of psychoanalytic education to the vertical by suggesting that diversity in supervision is a strength rather than a weakness. Schlesinger has his moment of reprise in the penultimate chapter. He admits to some of his own "blind and dumb" spots in a gracious fashion, showing how the study affected him. He also sticks to his impression that the merits of the study outweigh the disadvantages incurred by an untried method.

Shevrin is given the privilege of closing the book by returning to the point at which it began for him—when he was informed of the existence of the study. He moves beyond the personal and tries to give a condensed sense of what he found to be successful and unsuccessful in the supervisory experience. Sharp differences with Schlesinger remain. For example, he is convinced that the understanding

of the transference and countertransference issues in the supervision was blurred because the secret agenda of the study affected the supervisory relationship. He fittingly closes by reminding the reader that there was a patient whose problems made this study possible, and he tells what happened to her. Emerging from the depths of theory into the light of reality, one is impressed by the fact that the patient got better despite the imperfections of the treatment.

This book presents analysts with a unique opportunity to debate the difference between data and action research as a means of extending our understanding of what we do. To put this another way, the book is an exercise in hermeneutics. A text, severed from its sources in patient, analyst, and supervisor, is presented as a final version—a dead object—to a group of skilled interpreters, and they go to work to provide their own exegesis of it. The data acquires a life of its own as it is reorganized to extract new patterns and meanings. The ingenuity of the reorganizations is staggering, and the number of hypotheses emerging about what happened in the supervision is astonishing, but it remains an in vitro study of dead tissue. Psychoanalysts need to ask themselves how the hermeneutic approach can be useful clinically. How can it help us to know more about what to do? If we are battered by lists of information, summaries of tasks we are to perform, and abstractions of the goals we are to achieve, are we able to reapply the fruits of hermeneutic studies to the original object of the study?

Shevrin claims that hermeneutics or in vitro studies create blind spots that render exegesis inapplicable to him. Ekstein, implicitly, if not explicitly, is aware of this issue when he notes that parallelisms develop and go uncorrected as an inevitable consequence of the lack of feedback into the living processes of supervision and analysis. The unsolved problems in the patient-analyst dyad are repeated in the analyst-supervisor dyad and then in the supervisor-Study Group "dyad." [1] Since the most important problems are repeated rather than resolved, it seems apposite to raise the question why they should not be studied within the living processes rather than passed along in the form of data to be studied in isolation. Is not the purpose of supervision to prevent this regress from occurring by helping the candidate to deal with the problems

[1] *Cf.*, Sachs, D. M. & Shapiro, S. (1976). On parallel processes in therapy and teaching. *Psychoanal. Q.*, 45:394-415.

between him and the patient which he is not able to manage alone because of inexperience? To ascertain whether an idea that comes up in supervision is valid, the candidate can be helped to translate it directly into action so that the results can be studied via the patient's subsequent associations. When hypotheses cannot be tested in this way they can proliferate *ad infinitum*.

Although there are ways to usefully reapply what is learned in this way, it would be even more useful to strike a balance between hermeneutic study and the type of approach that Ekstein calls action research conducted directly within clinical work. At bottom is the unaddressed issue of which method of supervision best transmutes the candidate's communications into data suitable for the supervisor to use to infer meanings about blind and dumb spots. When this is confronted, a pathway to action research on supervision may emerge.

By challenging analysts to answer the many questions raised about the methods and goals of supervision, this book makes a contribution of great importance to psychoanalytic education. COPE should be pleased.

I have reserved one major quibble for the end. Since the book will be read mainly by people with old eyes, please, International Universities Press, use a larger type size in future editions!

DAVID M. SACHS (PHILADELPHIA)

THE MASK OF SHAME. By Léon Wurmser, M.D. Baltimore/London: Johns Hopkins University Press, 1981. 345 pp.

Léon Wurmser has set out to replace certain current psychoanalytic concepts of narcissism and structural narcissistic deficits with a much older, but seldom explicated line of investigation: the study of the affect of shame and its role in the conflicts of the psyche. One hopes that he has published this book at the right moment, i.e., at a time when disillusionment may have set in with parts of recent theories of narcissism, and the professional community may be ready for an original form of an old but useful paradigm. The author, who is erudite and well versed in the history of psychoanalytic thought, may be the person to achieve this goal.

Wurmser himself is more cautious than this in his assertions, stating that he wishes only to change the perspective a little, that

he has no quarrels with anyone, and that he can appreciate the validity of current concepts, including those of Kohut. Therapeutically, however, he sees much more value in examining the psychic conflicts of patients, no matter how ill, than in ransacking the wastebasket of developmental deficit.

As everyone knows, psychoanalysis has gone through phases of shifting theoretical emphasis. Psychoanalysis never totally gives up old concepts, but in practice periodic emphasis on new theories has often buried old ones. Thus we have had trauma theory, then an unconscious drive, fantasy, and affect phase, then an emphasis upon oedipal conflicts and castration anxiety, followed shortly thereafter by a newer concept of anxiety and the structural theory. A brief period of attention to the superego was followed by a longer period in which the ego was studied in its various functions. During the latter period it seemed to some analysts that unconscious fantasy, with its underlying drives and the defenses against them, began to recede into the background, although the aggressive drive and neutralization of drives received their share of attention. In the past fifteen years, the self and disturbances of the self have come into prominence under the rubric of "narcissism." Self- and object representations have become the object of study, often with a static "deficit in structure" emphasis in which psychic conflict has been de-emphasized. As Wurmser aptly points out, with this came diagnostic hyperbole in which more and more patients came to be labeled as borderline or as suffering from a narcissistic personality disorder.

With this new emphasis, some students of psychoanalysis lost their perspective on regression, defenses against affects, and the whole realm of sexual life which so many had found to be troublesome during previous periods. In studying shame, Wurmser brings us back to this dimension of life. It is not possible to study shame without examining sexuality, masturbation, penetration and, especially, masochistic submission. Nor can one ignore the wishes to look and be looked at and the powerful defenses against such wishes. Here we get to the wish to hide and disappear, the need to show no emotion lest one reveal weakness, or the need to deny feelings of shame by acting shamelessly, only to have shame break through in displaced, less sexual areas involving friendship or sincerely held beliefs. Of special value is Wurmser's emphasis on

conflicts over revealing the masochistic thoughts and wishes which underlie struggles involving penis envy and castration fear.

Wurmser describes shame from its most superficial to its deeper, unconscious levels. He examines it from the point of view of the most archaic remnants in the psyche as well as of its more organized, conflictual aspects. He offers clinical examples which illustrate various kinds of pathology in different patients, and exhibits a therapeutic optimism which is refreshing. He shows us that underlying the sexual content of shame reactions are feelings of defectiveness, dirtiness, and weakness, behind which is a basic fear of being unlovable.

In dealing with the more archaic elements of shame, Wurmser develops new theoretical elaborations on what are usually called exhibitionism and scopophilia. He believes that in the formation of object relations and object constancy, *looking* at the face, seeing its expression, and having one's own facial expression understood may be more basic than interaction via the mouth during the so-called "oral phase." When things go wrong in this sphere, fundamental shame pathology develops, centering around feelings of unlovability. He elaborates on the idea of the magic eye that can destroy and on the tendency to use masks in order not to be seen. He ties looking to thinking and magic and concludes that the need for privacy of thought is as important as the need for self-expression and for being understood. He ends by developing the idea of two drives which he calls theatophilia and delophilia (roughly translated as the wish to be looked at and a more active showing oneself to be looked at).

Wurmser cannot tie these newly defined drives to any specific erogenous or somatic zone. However, since it has become clear that the formation of attachments, bonding, symbiosis (or whichever term one wishes to use), is basic to human development, we need to think seriously about the formation of the mother-infant bond. On the basis of the work of Escalona and others, Wurmser concentrates on the eyes and the facial musculature. He makes it clear that the affect of shame develops by about eighteen months when the child is in the rapprochement phase and the anal phase, and when genital awareness is beginning to be present.

Although theatophilia and delophilia are central concepts for Wurmser, it is by no means certain that these terms will catch on

in a meaningful way for others. As Bettelheim has stated in a recent article in *The New Yorker*, complex Greek terminology is cold and indirect and makes human conflicts hard to grasp, since the word inventions stand between us and our feelings. Wurmser in general is careful not to employ cold technical abstractions, but in his thoughts on voyeuristic-exhibitionistic urges, as well as in his chapter on the magic eye, he excepts himself from his own rule.

Wurmser emphasizes that shame conflicts have content as well as form. He stresses the traumatic origin of many shame conflicts in actual experiences, with the trauma or the defense against it re-enacted over and over again in a later "fate neurosis" or some other form of pathology. This observation is not new, but the role of actual experience has receded in psychoanalytic emphasis, to be replaced by a stress upon the role of fantasy, as though fantasy does not often arise out of actual experience. Wurmser's point of view reminds us of the need for construction and reconstruction in psychoanalysis.

As the ego psychologists of the 1940's and 1950's did before him, Wurmser tries his hand at separating shame from guilt and shame from lowered self-esteem. Guilt and shame both are defined as deriving from superego conflicts, but they are defined further by the feeling states and content involved in each of them. Guilt arises from agressive actions or wishes to take such actions while shame arises from exposure of weakness and unlovability, according to Wurmser. This kind of definition in simple clinical terms has much to be said for it. There is little justification for these affects to have caused so much confusion in the past.

Wurmser describes ways in which shame can cause acute anxiety, traumatic anxiety, or signal anxiety. It also can contribute to chronic attitudes and chronic defenses or reaction formations, i.e., to character structure. Defenses against shame differ from defenses against guilt, in that there is much more stress on hiding and showing. In more severe forms of pathology, there can be paranoid-like feelings, depersonalization, and denial of reality, or, conversely, certain kinds of acting out, e.g., drug addiction and "shame psychosis," a term coined by Wurmser.

Wurmser differentiates between lowered self-esteem and shame by stating that in the former it is not necessary to experience self-punishment. Shame arises when the superego punishes with self-

contempt, which is not always present when there is lowered self-esteem.

Wurmser's definitions are important in viewing his struggles with the current narcissistic school of thought. It has become fashionable to use the terms "narcissistic blow" or "narcissistic hurt" instead of the word shame, which has been given a narrow meaning in terms of conflicts about genital exposure. But as occurs so often with language, the term "narcissism" has traveled a good distance from its early usage. As it tends currently to be used, it seems to refer mostly to fixation to pathological object and self-representations. We might well go back to a broader meaning of the terms "shame" and "narcissism." Shame is an affect that occupies the same realm as guilt and depression. We can describe its causes, ramifications, and metapsychology. The affect is irreducible, save into other affects. It is associated with bodily sensations and physiological manifestations.

What Wurmser does not discuss is why "shame" is so rarely referred to in psychoanalytic writings and case histories. Does the word itself raise in us an immediate defensive reaction? Does it hit us, like certain Anglo-Saxon words, below the navel? Are there countertransference reactions to our patients' sense of shame, because we feel we are invading their privacy and inviting painful self-exposure? Wurmser states that shame is experienced or defended against in almost every psychoanalytic session. Psychoanalysts are not immune from the daily onslaught of such emotions. It is probably true that the affect of shame communicates itself to the analyst in much more directly empathic ways than guilt or other anxieties.

This reviewer believes *The Mask of Shame* to be an important book which deserves to be read. There are flaws in it, however, which make it less successful than it could be. One basic flaw is introduced directly in the preface in the inference that Wurmser and his editors underwent disagreements which Wurmser won. Wurmser's solution is to ask that the book be studied rather than merely read. This is probably good advice for all important books, but Wurmser would have done better to have made use of good editing rather than leaving the reader with the task of wading through heavy, Germanic sentences, thoughts that come together at times only after several chapters, unnecessary peripheral allusions to literature or the Bible, chapter summaries that do not always

clearly epitomize the chapters' contents, and long case histories. The histories do not initially engage the reader. The book gets better as one reads along, but since Chapter I is particularly unappealing and most people tend to read a book in sequence, there can be an unfortunate effect on the reader. Perhaps the reader should come back to the early chapters after having started with Chapter VIII if one is clinically minded, or with Chapter VII if one would like to begin with an extraordinarily good review of the literature on shame and a summary of Wurmser's basic theoretical approach. Those who like simple, well-thought-out definitions of affects might start in the middle of Chapter II.

There is much that is well explicated and summarized in this book. If one keeps the above warnings in mind, *The Mask of Shame* could well be given priority on any list of new books for psychoanalysts.

LORE REICH RUBIN (PITTSBURGH)

ADVANCES IN SELF PSYCHOLOGY. Edited by Arnold Goldberg, M.D. New York: International Universities Press, Inc., 1980. 562 pp.

This book derives primarily from contributions to a conference on self psychology which took place in Chicago in 1978 under the sponsorship of the Michael Reese Hospital. Of the twenty-seven contributors, all but eight are either from the Chicago area or from the Cincinnati Psychoanalytic Institute, which originally was a satellite training center established under the sponsorship of the Chicago Institute. The book essentially expresses the authors' indebtedness to and appreciation of Heinz Kohut for his new psychology of the self. The spirit of the book is unabashedly evangelical, the aspirations apocalyptic.

At this late date, what more can one add to the already voluminous discussion of self psychology? Kohut's books have been widely read and intensively discussed. All the leading psychoanalytic journals have published articles by adherents as well as by critics of his views. The issues, theoretical and technical, have been weighed by some of our most distinguished colleagues. Recently, for example, an entire issue of the *Journal of the American Psychoanalytic Association* was devoted to the subject of the self. In it is a comprehen-

sive and masterful summary by Rangell.[1] Certainly no one can say that Kohut has been disregarded or that his ideas have not been given their day in court.

The range of contributions to this volume encompasses every aspect of psychoanalysis—theory, technique, development, and applied analysis. Each reader, accordingly, will concentrate on those papers that deal with the issues that interest him the most. In the case of the present reviewer, these happen to be the ones which deal with problems of methodology, its influence on the technique of therapy, and its relationship to the validation of interpretations and analytic hypotheses.

The clinical application of self psychology is organized around the report on a patient treated for three years by Evelyne Schwaber with an ostensibly favorable outcome. The presentation is straightforward and frank. The report of the patient's productions is followed by that of the therapist's interpretations and of her reflections on the basis for her choosing to intervene as she did. The material is not intended to serve as evidence in support of any particular thesis. It is meant to illustrate how the concepts of self psychology are applied in actual practice. Judging from the favorable reaction of several of the other contributors, including Kohut, it would seem that Schwaber fulfilled her purpose admirably.

The material reported and the manner of its treatment differ widely from what the so-called classical analyst is accustomed to hearing during supervision or reading as a case presentation. Excerpts from the patient's productions are brief, rarely exceeding fifteen short printed lines. No session is presented in its entirety or even in its majority. Consequently, it is not possible to ascertain whether a special theme is developing in the material. It is not possible to follow the interplay between impulse and defense, or to trace the derivatives of an unconscious fantasy. The data with which we are provided are immediate, declarative, and existential. Most of the dreams cited in the presentation are interpreted after few or, more often, no associations at all from the patient.

One should not think that this approach eventuates from any oversight on the presenter's part. Quite the contrary—it is carefully thought out and consciously applied. To begin with, Schwaber,

[1] Rangell, L. (1982). The self in psychoanalytic theory. *J. Amer. Psychoanal. Assn.*, 30:863-891.

like most of the participants in the conference, takes a dim view of the utility of the drive-defense model (i.e., the concept of intrapsychic conflict), especially with patients in whom disturbances of the self are paramount. In a carefully conceived, perceptive discussion of Schwaber's presentation, Kenneth Newman states: "Kohut's investigations have broadened our understanding of the disturbances that result from the thwarting of needs by unempathic, insufficient, or unavailable parents. Patients who present with a variety of symptomatic and chronic character complaints can now be understood to be suffering less from conflicts explained in terms of a drive-defense model than from self pathology—pathology that renders them vulnerable to alterations in self-cohesion and that is catalyzed in particular by empathic failures on the part of needed selfobjects" (pp. 263-264).

Subsequently, Newman goes a step further: "With our increased grasp of the central position of the self as the ultimate organizer in drive initiation and integration, we have recognized that common drive manifestations, at times appearing in crude sexual forms, should not be considered the core of the pathology. In many cases where self pathology is pivotal we will see the expression of drives as an attempt to remedy a disordered self, i.e., to counteract the feelings of inner depletion" (p. 268).

To Schwaber, however, the really significant element in organizing observations into meaningful data derives from the all-important role of empathy. In fact, she says: "In my presentation of the case of Mr. R. I was not focusing specifically on matters of technique or on nosological distinctions. I was not arguing for the selective applicability of the self psychological model to this patient. Rather, I was arguing for its general applicability—the utilization of a particular perceptual stance—the observer within the system, the 'contextual unit,' the heightened attention to the surround, the sharpened focus on certain immediate experiential phenomena. As I have indicated, it is this mode of observation which defines the psychoanalytic concept of empathy. The analyst, placing him or herself into the other's intrapsychic reality, views himself as being used and being responded to as part of the context of that reality. . . . Empathy, then, is implicitly used here as a mode of perception, not as a technique per se" (p. 254).

How Schwaber uses this mode of perception may be seen in the

following. Her patient, Mr. R., is a photographer whose main interest is in becoming a film maker. In this connection, she says: "Mother's looks, the failed 'gleam in her eye,' [a phrase borrowed from Kohut] *may be* the forerunner of Mr. R.'s camera interest. We can *conjecture* that the eye was hypercathected as touch was minimized. In his wish to 'freeze' a look, to capture a moment's look forever in its slightest nuances, which he felt especially when in a detached state, the camera served in some way to maintain a merger—as a medium for connection. He spoke of the eye as a camera" (p. 233, italics added).

Schwaber is most fortunate in her discussants. The contributions of Tolpin, Newman, Bach, and Shore are well thought out and clearly expressed. These contributions reflect on important issues involving technique, the need for disciplined empathy, and the role of defenses and unconscious fantasies even in self psychology. With regard to the last, Bach's presentation is a poetic tour de force. Nonetheless, Bach's juxtaposing of the concept of intrapsychic conflict to the developmental orientation of self psychology is totally unacceptable to Kohut: "[Bach's] critique of our clinical theory, however, is, to me, ill-founded; it is an outgrowth of the fact that he has not sufficiently reevaluated the limits of the applicability of the traditional belief in the universal clinical relevance of the oedipal configurations in the light of the significant new clinical data self psychology has furnished over the past decade" (p. 510). And further, "Instead of recognizing, however, that a crucial developmental need had been reactivated in the analysis, that a fragmented self had once more reached out for the opportunity to become cohesive with the aid of a guiding ideal, he was, in my view, misled by his inclination to order the data of his observation in accordance with oedipal patterns" (p. 511).

When empathy is treated as a form of direct perception and is not checked cognitively,[2] it may convey a sense of immediate conviction of reality, such as we bestow upon the sensations we receive from our perceptual apparatuses. Under such circumstances the search for confirmatory data may be discouraged during therapy and the rigors of scholarship may be bypassed in applied psychoanalysis. This is particularly pertinent to the section, Self Psychology and the

[2] Beres, D. & Arlow, J. A. (1974). Fantasy and identification in empathy. *Psychoanal. Q.*, 43:26-50.

Sciences of Man—easily the least convincing section of this volume. The contributions to the book in general, and to this section in particular, abound in quick, facile, capsule analyses of figures from literature and history and of events and social movements, all on the basis of little scholarship and much empathy. The model for this approach may have been borrowed from Kohut himself, inasmuch as in his comments he moves confidently and authoritatively from one analysis to another of the self problems of Freud, Napoleon, Mozart, Eugene O'Neill, and many others.

Even psychoanalytic scholarship tends to be neglected. Strozier, for example, as he discusses the interplay of narcissistic factors between the charismatic leader and the group, makes no mention of Christine Olden's paper [3] on the fascinating effect of the narcissistic personality which, in 1941, addressed the very same problems. Similarly, in all the discussion of empathy, hardly any reference is made to the rather considerable volume of literature that has appeared on the subject in the past, with the exception, of course, of Kohut's 1959 paper [4] on that subject. As a result of these omissions, there is relatively little reference to the subject of countertransference, a very significant factor that has to be considered in all discussions of empathy. In this entire volume, not one author has occasion to refer to Annie Reich's "Narcissistic Object Choice in Women" [5] or to her paper, "Pathologic Forms of Self-Esteem Regulation" [6]—two papers widely regarded as classics in the field of self psychology.

There is perhaps nothing that brings into sharper focus the divergence of self psychologists from the empirical methodology of psychoanalysis than their attitude toward the developmental studies of Spitz and Mahler. Over the years, these two investigators, especially the latter, have carried out meticulously documented clinical and developmental studies of very young children. It would be hard

[3] Olden, C. (1941). About the fascinating effect of the narcissistic personality. *Amer. Imago,* 2:347-356.

[4] Kohut, H. (1959). Introspection, empathy, and psychoanalysis. An examination of the relationship between mode of observation and theory. *J. Amer. Psychoanal. Assn.,* 7:459-483.

[5] Reich, A. (1953). Narcissistic object choice in women. *J. Amer. Psychoanal. Assn.,* 1:22-44.

[6] Reich, A. (1960). Pathologic forms of self-esteem regulation. *Psychoanal. Study Child,* 15:215-232.

to find any researcher in the field who can be said to be more sensitive than Mahler to the part played by empathic interactions, both in the realm of child observation and in the developmental structuring of those elements that facilitate the emergence of the self. It is striking to observe, therefore, that in the closing section of this book there appears a sharply dismissive response by Kohut to a suggestion offered by a colleague that Spitz's idea of growth-promoting separation and Mahler's concept of the beneficent effects on the self of the empathically attuned mother could be reconciled or integrated with self psychology. Kohut finds that he cannot agree with such a proposition, because such findings "do not lie squarely within the realm of that aspect of reality that is accessible via introspection and empathy . . . [therefore] I cannot get a firm grasp of their psychological meaning" (p. 450). He goes on to say that "the framework within which Spitz's and Mahler's theories belong and within which their statements find their meaning is the framework of sociology" (p. 452). Mahler's observations are not necessarily wrong, but they are "experience-distant because they are not derived from the prolonged empathic immersion into the inner life of the observed but from the scrutiny of behavior that is evaluated in accordance with traditional value judgments of Western man" (p. 452).

In her zeal to establish priority and authenticity for Kohut's theories on self-cohesion and selfobjects, Marian Tolpin restates Mahler's position in terms that may be considered both inaccurate and misleading. In a footnote Tolpin states: "The *developmental writings* of Mahler and other ego psychologists who are also adherents of object relations theory recognize the importance of the formation of psychic structure. However, failures of cohesion, integration, etc. are nonetheless attributed to conflict (cf., for example, Mahler et al., 1975, pp. 214, 222). Their *clinical theories,* on the other hand, disregard the problem of structure formation and continue the emphasis on the primacy of conflict" (p. 49, n.). Here Tolpin has blurred the distinction between dialectical contradiction and structural conflict. States of distress are not to be compared with structural conflict as encountered in psychoneurotic symptomatology. Experiences of unpleasure and states of distress, helplessness, etc., can have both a disorganizing *and a progressive effect* on structure formation. Uneven development and developmental lag,

as repetitive or even necessary circumstances for developmental advance, have been noted by many observers, including Piaget.

Nor is it accurate to suggest that Mahler and her collaborators disregard the concept of prestructural psychology or basic ego defects,[7] or that they assume that infantile rage over helplessness of necessity leads to conflict psychopathology. In their enthusiasm for the selfobject concept, self psychologists frequently overlook the relevance and the importance of the pleasure-unpleasure principle. It also strains credibility to say that "Mahler's theory overlooks psychoanalytic knowledge of the fact that there is inevitable frustration from birth on, along with the fact that the idea of sudden overwhelming frustration from cognitive maturation does not square with the reality of everyday small frustrations" (p. 53).

On the other hand, the assumptions posited by Kohut, with hardly any basis in developmental observation, are accepted uncritically by Tolpin: "Normally equipped with expectable endowment and an expectable selfobject environment, Kohut's baby experiences itself and is experienced as a strong, effective, independent center of initiative, capable of achieving and accomplishing its own (in-phase) purposes and goals. . . . When reasonably attuned selfobject responses meet the baby's active initiative and his normal expectations part way, his inherent vitality is simply preserved; and when this is the case, he automatically continues to exercise to the hilt all of his progressively growing and unfolding capacities and all of the expanding signals and signs at his disposal in order to continue to assert himself and to announce his legitimate developmental needs" (p. 55).

Question: How does one arrive at this knowledge of and insight into the mind of a young infant? Answer: By introspection and empathy.

Not even a long essay can do justice to the many issues, problems, and insights raised in this book. The section on self psychology in development is crucial in establishing the self-observational basis for the theories of pathogenesis advanced by the self psychology school of thought. I stated in the beginning that every aspect of psychoanalysis is examined and considered from the vantage point of the new paradigm of self psychology, a psychology that differs

[7] See, for example, Weil, A. P. (1978). Maturational variations and genetic-dynamic issues. *J. Amer. Psychoanal. Assn.*, 26:461-491.

fundamentally from accepted concepts of pathogenesis and therapy. The emphasis is on early events; the accent is biological and developmental. The vicissitudes of narcissistic development of self-esteem regulation as affected by empathic or unempathic parenting are held to be crucial.

Self psychology espouses a very specific image of man which tends to evoke a sympathetic response, as Ticho has observed,[8] in those who emphasize in their concepts the subjective, creative, experiencing aspect of the psyche. The idea of health in self psychology holds out the prospect of human and individual perfectibility. As Kohut puts it, "A psychologically healthy person, we may say, will live out the particular design that is laid down in the center of his self and achieve his particular nuclear productivity or creativity whatever the attitude of society to his actions may be, whether accepting or rejecting, approving or disapproving" (p. 498).

Any reader of this volume cannot help but be sensitized to areas of psychology which have been relatively neglected in the past. For the reader who wishes to learn at first hand what the current concepts of self psychology are and how they are applied in actual practice, there is no better volume than this one.

JACOB A. ARLOW (NEW YORK)

INTELLIGENCE AND ADAPTATION. AN INTEGRATION OF PSYCHOANALYTIC AND PIAGETIAN DEVELOPMENTAL PSYCHOLOGY. (Psychological Issues, Volume XII, Nos. 3/4, Monograph 47/48.) By Stanley I. Greenspan, M.D. New York: International Universities Press, Inc., 1979. 408 pp.

The significance of Piaget's researches into developmental epistemology has long been apparent to psychoanalytic investigators. A number of psychoanalysts have constructed bridges between various aspects of Piaget's observations and formulations and psychoanalytic theory, but until now no one has been ambitious enough to attempt a total integration of the two sets of theoretical constructions.

In this volume, Greenspan has gone beyond his earlier effort to elaborate a detailed "psychoanalytic learning perspective"[1] by

[8] Ticho, E. A. (1982). The alternate schools and the self. *J. Amer. Psychoanal. Assn.*, 30:849-862.

[1] Greenspan, S. I. (1975). *A Consideration of Some Learning Variables in the*

attempting just such a total unification of the psychoanalytic and Piagetian approaches to human development. That he has attempted to effect a productive dovetailing between two scientifically valuable bodies of knowledge in itself merits praise. That his labors have yielded intermittent flashes of light that illuminate certain aspects of the developmental process in a heuristically useful way warrants an expression of gratitude. That his overall effort to integrate psychoanalytic theory with Piagetian developmental epistemology has been only partly successful, however, is not surprising. It simply is not possible to effect a total unification of these two bodies of theory which, despite certain points of overlap and similarity, diverge so widely in their respective fields of observation and in their areas of inquiry.

Greenspan is quite on the mark, in fact, when early in the book he addresses himself to a number of areas of incompatibility between the psychoanalytic and Piagetian points of view. He calls attention in particular to the disparity between Piaget's narrow focus on cognitive development, observed under optimal conditions of minimal tension and relative freedom from conflicts, and psychoanalytic investigation into a very broad range of human development, functioning, and behavior in the midst of all the storms of emotional strain and conflict that characterize the human condition: "In contrast to psychoanalysis, which studies the human, affect-laden environment (in all its variation and unpredictability), Piaget studied the non-affect-laden, often nonhuman environment. . . . This narrower field of study has permitted Piaget to generate a highly precise model of cognition" (p. 38).

It is no wonder that Piaget disputes the importance and even the existence of internal drive pressures and emotional conflict as significant forces shaping development ("Piaget essentially has no theory of motivation" [p. 96]), for he has carefully excluded them from his field of observation. As Greenspan states: "It is important to point out a crucial difference: Piaget clearly has not accepted the dynamic point of view of psychoanalysis" (p. 101).

Greenspan notes that when an infant or child contemplates inanimate objects in his environment, he contends with a much less

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Context of Psychoanalytic Theory: Toward a Psychoanalytic Learning Perspective. Psychol. Issues, Monogr. 33. New York: Int. Univ. Press. Reviewed in *This Quarterly,* 1977, 46:688-690.

variable and more stable set of stimuli than when he interacts with the world of animate objects. Piagetian observations focus upon much simpler phenomena than does psychoanalytic investigation. To attempt to correlate and compare psychoanalysis and Piagetian psychology, let alone integrate them, is a problematic undertaking.

Yet this is precisely what Greenspan sets out to do in this book. He bases his integrative efforts upon two areas of compatibility he sees between psychoanalytic theory and Piaget's developmental epistemology. The first is a similarity between Nunberg's description of the synthetic function of the ego and Piaget's formulation of assimilation and accommodation as the basic sensorimotor mechanisms underlying the earliest cognitive activities. This reviewer finds Greenspan's reduction of the ego's synthesizing activity to *no more than* assimilation and accommodation (pp. 46-47) forced and unconvincing. When he comes very close to reducing defense activity in general to vicissitudes of assimilation and accommodation (pp. 47-49), I find myself even more uneasy. While I agree with Greenspan that the study of cognitive development is of great value for psychoanalysis,[2] I cannot agree that it is possible to understand either defensive or conflict-free psychological activity in purely cognitive terms.

The second area of compatibility Greenspan uses as an integrative springboard is that between Piaget's "constructivist structuralism" (p. 59) and the psychoanalytic concept of structure, as defined by such psychoanalytic theoreticians as Rapaport (who is cited extensively, while Hartmann, Kris, and Loewenstein are given scant mention, and Freud's later ego psychological contributions are all but ignored). Greenspan calls attention in particular to the observations that "some structures are present at birth . . . [and] that all psychologically relevant structures have a developmental sequence" (p. 114), as well as that some structures determining behavior "may be relatively autonomous from the drives" (p. 115). In his attempt to bridge psychoanalytic theory, with its emphasis upon dynamic and energic hypotheses, with Piaget's nonmotivational, structuralistic approach, Greenspan cites Hartmann's and Erikson's elevation of

[2] Silverman, M. A. (1971). The growth of logical thinking. Piaget's contributions to ego psychology. *Psychoanal. Q.*, 40:317-341; Silverman, M. A. (1981). Cognitive development and female psychology. *J. Amer. Psychoanal. Assn.*, 29:581-606.

adaptation to a status of importance (p. 117), Rapaport's "observation that the process of neutralization changes the relationship between primary and secondary process functioning" (p. 121), and the observation that with advances in ego psychology "the drives are no longer thought of as the ultimate or only determinants of behavior, but as one set of determinants in interaction with other sets" (p. 120).

"Piaget offers psychoanalysis," Greenspan writes, "a rather specific developmental model of the particular substructure of the ego that constructs, by secondary-process thinking, an adaptational relationship with the environment" (p. 126). Greenspan attempts to coordinate this model with psychoanalytic theory "to delineate the relationship between the drives and the development of intelligent behavior" (p. 126). He adduces the "simple example" of a young latency child grappling with unconscious, oedipal, murderous wishes toward his father to demonstrate "that an understanding of cognitive structures may facilitate understanding of the ego's defensive operations against drive derivatives" (p. 127). Greenspan suggests that Piagetian cognitive psychology can explain such a youngster's defensive maneuvers in terms of the acquisition of "the capacity to organize his cognitive operations in systems whose transformational laws depend on reversibility by inversion. . . . It will be hypothesized later that this new cognitive capacity leads to differences between the latency and prelatency child in handling concerns with angry feelings. The latency child can change a wish into its opposite (the inverse relationship), while the prelatency child does not have this cognitive capacity and may therefore be limited to denial of the wish" (p. 127). I find this not only simplistic but inaccurate. Where, for example, do the reaction-formations of the preoedipal and early oedipal child fit into this formulation? Greenspan does proceed later on in the book to a much more sophisticated and useful formulation of the role of advance from preoperational to operational thinking in shaping the defensive (and conflict-free) functioning of the latency child; but he fails entirely to mention that Sarnoff [3] has already carried out such an investigation, arriving at essentially the same conclusions expressed by Greenspan. A serious weakness of Greenspan's book, in fact, is that the review of the literature (with John Curry) is extremely brief (covering only

[3] Sarnoff, C. (1976). *Latency*. New York: Aronson.

twenty pages), at times is glib, and does not do justice to the work done by previous investigators who have attempted to integrate relevant aspects of Piaget's observations into psychoanalytic thinking.

There is another, related discrepancy in the book. Greenspan devotes an overwhelming preponderance of attention to demonstrating the relevance of Piaget's observations and ideas to psychoanalysis. But he makes much too little effort to examine the ways in which psychoanalytic discoveries about the world of emotions might clarify and expand the understanding of cognitive development that is obtainable with Piaget's investigative approach and methods.

This brings us to Greenspan's attempt to effect a global integration of Piagetian and psychoanalytic psychologies. He finds some of the apparent incompatibilities between psychoanalysis and genetic epistemology to be more virtual than real. He ascribes Piaget's objection to the notion that frustration and heightened drive tensions play a part in motivating the development of symbolic thought to excessive literalism and lack of appreciation of theoretical sophistication productive of such concepts as primary autonomy and an initial, undifferentiated ego-id matrix. He is not put off by Piaget's failure to consider internal experience and his opposition to the ideas of unconscious motivation and a dynamic unconscious. Taking the point of view that Piaget has merely restricted himself to study of the *external boundary* of the ego, he proposes that application of Piaget's developmental principles to the shape and form of the ego's *internal boundary,* i.e., its relationship with the drives, would expand and enhance both theories. In doing so, he subsumes "Piaget's model of cognitive development as a subsystem of the psychoanalytic model" (p. 99).

Greenspan's attempt to harmonize the dynamic point of view of psychoanalysis with Piaget's map of the course of cognitive development stresses the infant's dependence upon the mothering figure for a highly discriminative feedback of a very high order of stability and reliability as ego development proceeds on a hot, drive-affective level at its "interior boundary." This contrasts sharply with the infant's simple reliance upon reproductive assimilation at the "external boundary" of cold, unemotional interaction with relatively stable, invariable, inanimate environmental objects.

He applies Piagetian principles in an interesting way to a theo-

retical examination of self-object differentiation, the development of detour mechanisms in the service of defense and of general ego growth, separation-individuation, and the mutual influences of cognitive and emotional experience at different developmental levels. He offers a useful expansion of the psychoanalytic notion that the effect of traumatization depends in part upon the developmental phase at which it takes place when he underscores Piaget's observation that advance from a lower to a higher level of cognitive organization is accompanied by a regressive floundering and increased vulnerability as the child approaches the world from a new, unfamiliar perspective (vertical *décalage*).

When Greenspan attempts to integrate Piaget's observations with those of psychoanalysis on the developmental struggles of preschool and school age children, however, he is quite disappointing. His exegesis develops, in fact, into little more than a detailed and repetitive summary of Piaget's ideas rather than into a true integration of cognitive epistemology with psychoanalysis. What little he offers in that direction is unexciting and not entirely convincing. When he addresses himself to the preoperational child, for example, he says little more than that Mahler has taken the level of cognitive organization into account in developing her ideas about rapprochement, that magical thinking in part is cognitively derived, and that cognitive development can be disturbed by traumatic separations early in life. He does not address himself to such key issues as ambivalence toward the mother, egocentrism, narcissistic-exhibitionistic inclinations, difficulty in comprehending excretory and toileting experiences, and the discovery of the difference between the sexes. The cognitive limitations of preoperational thinking are, in fact, enormously important in understanding these developmental phenomena. Greenspan's attempt to correlate psychoanalytic and Piagetian approaches to the oedipal child (who is undergoing transition between preoperational and concrete operational thinking) and the latency age (concrete operational) child is also weaker than it might have been. His explanation of reaction-formations and rationalizations in latency in terms of the acquisition of reversible, alembic cognitive operations, for example, comes across as forced and unconvincing. He merely alludes sketchily to the way in which narcissistic fixation or anxious regression to magical thinking can interfere with the crucial development of operational thinking

during latency, leading to permanent impairment of learning, reasoning, and overall ego strength and flexibility.

Greenspan's approach to the adolescent period, on the other hand, is quite interesting and useful. He describes clearly and convincingly how advance from concrete to formal operational thinking, with acquisition of the capacity to coordinate inversion and reciprocity in examining relationships, provides the adolescent with a vastly more flexible, balanced, integrated perspective, useful not only for apprehending the physical world in which we live, but also for dealing with complex, conflicting, mutually contradictory emotions and attitudes about onself and other people. Insofar as there is cognitive access to inner feelings, what had been unmanageable and therefore frightening can now be understood and harmoniously integrated rather than rigidly avoided (repressed), inverted (denied and projected), turned into its reciprocal (reversed via undoing and reaction formation), etc. Finally, Greenspan briefly attempts to weave psychoanalytic observations of the complex development and evolution of dyadic and triadic relationships into an integrated model of intelligent learning and intellectual development. Incomplete as it must be, his effort to coordinate development at the "external and internal boundaries" of the ego is commendable.

In sum, this is a book for those who want to learn about Piagetian genetic epistemology, are interested in psychoanalytic participation in interdisciplinary discussion and integration, or are interested in the developmental process and/or in intellectual development. Unfortunately, the description of Piaget's ideas is overlong and repetitious,[4] and readers who are familiar with them may be reluctant to wade through this material another time. But if they do so, they will be rewarded with ideas of enough heuristic value and thought-provoking interest to make it a worthwhile if somewhat tedious journey.

MARTIN A. SILVERMAN (MAPLEWOOD, N.J.)

[4] Several excellent short summaries of Piaget's observations on cognitive development are available. Among them are Piaget's own slim volume, *The Psychology of Intelligence,* Patterson, N.J.: Littlefield, Adams & Co., 1960 (originally published in French in 1947), and *The Psychology of the Child,* by Jean Piaget and Bärbel Inhelder, New York: Basic Books, 1969 (originally published in French in 1966). Another very good outline of Piaget's ideas is *An Introduction to Piaget,* by P. G. Richmond, New York: Basic Books, 1971.

LANGUAGE AND THE ORIGINS OF PSYCHOANALYSIS. By John Forrester. New York: Columbia University Press, 1980. 285 pp.

If asked to consider the influences which most profoundly operated upon Freud in his creation of psychoanalysis, most students of the field would mention the biological and physical sciences as Freud knew them. In this always interesting and frequently exciting book, John Forrester, a Cambridge University philosopher of science, offers the alternative thesis that Freud and many of his early followers were even more influenced by their understanding of language generally and, more specifically, by that nineteenth century branch of linguistic study known as philology. As the title suggests, to Forrester "language is the central concern of psychoanalysis." He attempts, as did Freud, to explain the way in which the curative process is related to the power of words. To assist in this rather large task, Forrester offers the reader a friendly critique of certain Freudian texts and letters, an extensive discussion of the history of aphasia and its paramount conceptual significance in the development of psychoanalysis, and reflections upon nineteenth century linguistics (especially philology), symbolism, grammar, and the concept of the unconscious. As the author correctly notes, it is not easy to find a disciplinary niche for this work. It is full of surprises for the traditionally educated psychoanalyst.

If psychoanalysis as conceived by Freud can be said to be a study of the unconscious and of the rules which govern it, it might not be unfair to characterize Forrester's work as an exploration of the theory of consciousness implicit in early psychoanalytic thought. The exploration begins with the observation that Freud had two different conceptualizations of the unconscious. One was the familiar metapsychological thesis that there is a system of unconscious ideas and a separate system of word presentations with which the unconscious ideas articulate. This emphasis upon the primacy of the unconscious contrasts with a second conceptualization in which the inferential nature of the unconscious and the primacy of word presentations in providing the empirical basis of psychoanalysis are emphasized. It is the second conceptualization, relating more closely to conscious phenomena and to clinical theory generally, that Forrester builds upon. Forrester offers evidence that Freud rejected the notion of a real distinction between ideas or words and the objects they represent. Thus, words became a road, if less than the

royal road, to the unconscious. As Forrester puts it, "what is present in Freud's early cases (and absent in those of others, such as Charcot), is a sense that the neurosis and its cure is determined by the patient's system of ideas, as expressed in words, in which the doctor becomes unavoidably enmeshed" (p. 13).

How the system of ideas and words becomes symptomatic is the next issue that Forrester takes up. He goes back to the theory of aphasia expounded by Hughlings Jackson in 1878-1880. This held that when an aphasia-producing lesion occurs, the patient becomes locked into the thought or utterance then being expressed. Thereafter, that utterance is repeated automatically, without logical reference points. A regression then occurs to a more highly organized, more primitive automatic state of the nervous system. In this stage language is "ready made up," there is less spontaneity and a reduced possibility of consciousness. The parallel to some of Freud's psychological concepts two decades later is striking. As Forrester puts it, "the chronic hysterical symptom . . . was a piece of language that had once had meaning but which, by becoming cut off from the dynamic structure of contingent elements . . . had lost its meaning, and . . . was doomed to repetition" (p. 20). Freud recognized that meaningfulness could be restored by placing the symptom within its traumatic historical context. To Freud, the "lesion" in hysterical paralyses was in the system of ideas rather than in the nervous system,[1] and it could be understood, in part, as a disturbance in the relations between specific word presentations and specific object presentations. Ultimately, the theory of neurosis depended on the occurrence of the first trauma before the age at which ideas could be put into words, and, as Forrester notes, by 1915 Freud held that it was the failure of translation into words which characterized a representation as unconscious.

The technique of free association as a method of depth psychological investigation also appears to have originated in aphasia studies. Freud's major contribution to the understanding of aphasia was his concept of a "zone of language." It is Forrester's view that by conceptualizing language as a translation of perceptions, which is different from and has little to do with the specific mechanisms that the nervous system uses, Freud essentially "psychologized"

[1] Freud, S. (1893). Some points for a comparative study of organic and hysterical motor paralyses. *S.E.*, 1, p. 169.

aphasia studies. This ultimately made possible the understanding of neurosis as a "lesion" which disrupts the nexus of language and ideas.

The centrality of language in the origins of psychoanalysis is taken a step further by Forrester when he confronts Freud's dual view of symbolism. On the one hand, Freud could think of symbols as part of the "collective linguistic unconscious," as vestiges of a time when sexuality and perhaps other basic human functions were identical with language. On the other hand, Freud was concerned that psychoanalysis not become merely the application of a dictionary of universal symbols. Within this polarity the oedipus complex emerged some eight or ten years after *The Interpretation of Dreams* as a full-blown structure developed out of the myriad of details—oedipal impulses—which Freud gradually uncovered in his self-analysis and elsewhere. To Forrester the oedipus complex is essentially a symbolic form that is not unlike other universal symbols in that it illuminates the deepest and most primitive of human experiences. Details of the analysand's life history gradually became less important than they had been earlier in Freud's clinical thinking; instead, the process of interpretation came increasingly to center upon aspects of the oedipus complex as the universal heritage of every human being. Seen from this interesting perspective, the concept of the oedipus complex has the advantage of parsimony and simplicity and the disadvantage of any universal symbol.

A chapter on grammar offers a number of interesting observations—not so much on the transformation of the thought of the patient according to grammatical rules as on consciousness itself. Forrester notes the mutual exclusivity of consciousness and memory in Freud's later thinking,[2] as opposed to Freud's earlier observation that "normal forgetting takes place by way of condensation [and that] in this way it becomes the basis for the formation of concepts".[3] So, just as consciousness replaces memory, concepts arise in place of memories of perceptions. Perhaps, Forrester suggests, infantile amnesia is due to the fact that speech is built out of and replaces the sensory experiences of the infantile period. In a parallel manner, the speech of the neurotic analysand replaces or opposes the unconscious memory and in itself produces a mutative effect. The language

[2] Freud, S. (1920). Beyond the pleasure principle. *S.E.*, 18, pp. 26-27.

[3] Freud, S. (1901). The psychopathology of everyday life. *S.E.*, 6, p. 134, n. 2.

used by the analysand, to be mutative, must not be a mere symptomatic repetition of the words at the core of the neurosis.

In the final chapter, on philology, Forrester rather convincingly draws together the various strands of his central thesis that species (and individual) language development contributes no less than biology to the origins of psychoanalytic thought. The task of the nineteenth century philologists, to find the basic or original language, was solved, in part, by Freud's discovery of infantile sexuality. That is, if the origins of language lay in words which related to sexual and other basic human functions, then the thought expressed by the theory of sexuality was a reflection of the most basic of human functions. In Freud's own psychoanalytic thinking, the abstractions provided by infantile sexuality could be used to fill gaps in the free associations of his patients, since, as a derivative of the most basic human thought and language, the terms of infantile sexuality could be applied to a patient as much as that person's own associations.

Beyond infantile sexuality as language, Forrester addresses the problem of the way in which the earliest linguistic traces are transmitted to the present as well as from person to person in some unconscious manner. He suggests that concepts such as *Volksgeist* or a "polyglot unconscious" (taken from Sebastiano Timpanaro), concepts which approach Jung's "collective unconscious" and which appear to us to be no less mystical (and to have no more supporting evidence), may explain the means of transmission.

This scholarly and quite convincing volume presents a facet of Freud's thought which, while not unknown to us, has never before been so well or so thoroughly documented. Neither has it been previously organized and placed within a context so potentially useful in advancing psychoanalytic theory. But *Language and the Origins of Psychoanalysis* is by no means a study of the totality of psychoanalytic theory, and it makes no claim to be. Forrester is not a clinician, and, while he does trace the evolution of Freud's use of language and symbol in extending his grasp of ever more complex clinical situations with great sensitivity, many elements of clinical psychoanalysis are hardly addressed. Among these are affect, the role of empathy, all nonhysterical somatic reactions, and, of course, clinical entities which do not result from psychic conflict.

Probably the most important yield of this point of view in terms

of contemporary issues is the support Forrester implicitly, and no doubt regrettably, gives to those who reject the central position of the oedipus complex in psychopathology. In placing the oedipus complex into the same matrix as symbol and primal language, Forrester forces us to consider that it, too, may be in part a universal symbol which will, like other universal symbols have in the past, become less useful as we acquire more understanding of development.

The very effectiveness of Forrester's argument makes the reader momentarily neglect many other facets of thought synthesized by Freud in creating psychoanalysis. One very obvious and important neglected area is the beginning of a developmental psychology, which has only in recent years begun to make contributions beyond those generated by Freud himself. More important is another limitation. As noted above, Forrester's whole thesis takes off from the second of the two Freudian conceptualizations of the unconscious, the one which lent itself most readily to a linguistic theory of psychoanalysis. Forrester's thesis may be seen as a theory of consciousness implicit in early psychoanalytic thought. It is very little concerned with the unconscious as a system of ideas separate from the system of word presentations. These are necessary limitations, which enable Forrester to concentrate his attention on language *per se*. However, the elements excluded or minimized by this focus, that is, that part of Freud's creativity which depended upon his empathic and introspective capacities and his aesthetic sense of the organization of the unconscious and of character structure, are more central to his development of psychoanalysis than is his philological approach.

The most serious theoretical difficulty inherent in Forrester's approach resides in its tendency to subtly reverse the distinction Freud made between language and thought, a distinction which Forrester notes. Language considered independently from introspection tends to become equated with thought, if for no other reason than that it becomes the only route to thought. In the single bit of dry humor to be found in this work, Forrester notes that philosophers, like schizophrenics, detach words from their thing representation. In this fashion, the words become equivalent to thought: the nonverbal level of mentation to which Freud referred as thing representation is more or less eliminated from consideration. It may

be that by this bit of self-mockery Forrester reveals his bias toward equating thought and language. Perhaps such a difficulty is inherent in any effort at textual analysis, since much of the rich flow of information that passes between the participants in a clinical interaction is simply not available in even the best of texts.

Another way to express how Forrester conceptualizes Freud's use of language is that he sees it as more Lacanian than hermeneutic, that is, that language was used by Freud more to arrive at primal universals than to interpret the productions of the patient in order to construct a coherent life story. The tension and reciprocity of influence between these two are of much less interest to Forrester than to the psychoanalytic historian concerned with the whole of Freud's thought.

Although this is not an easy book to read, I can strongly recommend it to the analyst-scholar for its thoughtfulness and the coherent expression it offers of the way in which language entered into the origins of psychoanalysis. The special point of view that is presented makes a great deal of sense. To repeat, this book is always interesting and is frequently exciting.

LEO SADOW (CHICAGO)

SOUNDINGS. PSYCHOHISTORICAL AND PSYCHOLITERARY. By Rudolph Binion. New York: The Psychohistory Press, 1981. 164 pp.

This volume contains six essays, written between 1959 and 1978 on the following topics: Kafka's *The Metamorphosis*; King Leopold III and Belgian neutrality prior to World War II; the deaths of Ludwig II of Bavaria, Crown Prince Rudolph of Austria-Hungary (at Mayerling), and Archduke Franz Ferdinand of Austria-Hungary (at Sarajevo); Hitler's policy of eastward expansion; doing psychohistory; and several Pirandello plays, most notably, *Six Characters in Search of an Author*. In the publisher's and, I gather, Binion's opinion, the essays "retrace the author's development in historical and literary scholarship from applied Freudianism to direct psychohistorical inquiry unencumbered by clinical models." In my own judgment, Binion takes one step forward, two steps back.

The Kafka essay appears to be an exercise in "applied Freudianism." Binion argues that Gregor Samsa has not been literally transformed into a bug, but rather is suffering from a psychotic delusion

of being metamorphosed. Samsa thinks he is a bug and acts like one; and because most of the narration is from his perspective, the reader shares his experience. But in Binion's opinion, the responses of the other members of the family suggest only that Gregor has been changed in some bizarre fashion, not that he has changed species.

I do not find this interpretation plausible, at least not in the absence of evidence that Kafka intended to present a psychotic breakdown. No doubt, Kafka had an unusual capacity to use the "psychotic" part of his own personality as raw material for his writing. But at a minimum, the compelling quality of *The Metamorphosis* rests upon our inability to determine if the transformation is literal or not. I also think the text reads better, in both mood and detail, when one proceeds on the assumption that in Kafka's inner world people can be turned into bugs. And if this assumption is correct, then to contain the story within the diagnosis of psychosis is to obscure its meaning, just as to confine human beings within the categories of psychopathology is to deny them their individuality. To state this another way, Binion's interpretation of *The Metamorphosis* is an entirely external or a priori one. He establishes a certain correspondence between a conceptual form (psychosis) and a literary content (Samsa as a bug). But we never enter into Samsa's world—or Kafka's.

The Kafka essay stands on a different methodological footing from the other pieces in the volume. The others involve the use of a more phenomenological method, through which Binion first finds a meaning or connection *within* a manifold of events and only then provides a psychological explanation for what he has uncovered. This approach is not without its hazards. On the one hand, it depends upon the investigator's intuitive and empathic capacities, which may or may not be adequate to the phenomenological task. On the other, just because phenomenological inquiry has this subjective dimension, there is always the possibility of finding only the meaning for which one is searching, or of placing into the phenomenon the meaning one later extracts from it. But this is only to say that, at its worst, phenomenological inquiry is a covert form of a priori theorizing.

Presumably, Binion's development from "applied Freudianism" to "direct psychohistorical inquiry" is represented by the shift from

the first to the subsequent essays. Binion claims to have liberated himself from conventional psychohistory and to be carrying a certain burden as a consequence:

> The usual practice, or malpractice [of psychohistory], is to read current clinical concepts into the historical record, and the more the merrier. To ignore the clinicians, to sound the past empathically, to isolate decisive causes: this is culpable for many a psychohistorian . . . (p. 5).

There is justice in the charge that psychohistory usually involves the mechanical application of psychoanalytic concepts. The phenomenological methods of clinical psychoanalysis can likewise be perverted through the analyst's desire to prove a theoretical point. But the misuse of concepts says nothing one way or the other about their validity. In either clinical or psychohistorical investigation one can, for example, impose an oedipal configuration upon or discover an oedipal configuration within the given subject matter. The question, in other words, is not a concept's place of origin, but rather the road it travels to reach its interpretive destination. There is no justification for statements like "not even tangential relevance can be found for the psychoanalytic theories of group processes drawn from self-observing artificial groups" (p. 125). The object of Binion's criticism here is W. R. Bion's *Experiences in Groups.* This book, along with Freud's *Group Psychology and the Analysis of the Ego,* is exceedingly rich in concepts which—if properly used—are illuminating of historical phenomena. Is it not possible, for instance, that the leadership cadre of a mass movement might act on the basis of unconscious fight-flight or dependency reactions while simultaneously claiming to serve rational collective interests? Further, if one examines the transference and countertransference processes in psychoanalytic groups, might not one generate hypotheses concerning the emotional impact of group members upon group leaders? And would not such an obvious hypothesis be that leaders often unknowingly respond to and enact wishes of the group as a whole?

I have taken the reader on this flight of conceptual fancy because Binion traces Hitler's policy of eastward expansion to the emotional interaction between the future Führer and his audience in the early 1920's. Unlike Binion's perception of Hitler's anti-Semitism, the roots of this policy, he feels, cannot be found in Hitler's personal experience. But when he imagined himself to be a member of Hitler's audience, he found an explanation: after "long months of

straining my mind's ear I could all at once hear, and shrilly, where his call for eastward expansion resonated with a traumatic national experience" (p. 123), i.e., the German defeat of 1918. This discovery Binion views as his "proudest research coup" (p. 123).

Binion may or may not be correct in his analysis of the origins of Hitler's eastern policy. But, in any case, his discovery fits neatly within the framework of Freud's and Bion's group psychology. Binion is correct in not imposing his framework a priori. But why deny its relevance when a group-emotional phenomenon emerges from the research process; when the psychoanalytic construct fits the phenomenologically generated data; and when the construct contains additional concepts which might lead one to and help explain other historical linkages?

The same kinds of questions can be raised about Binion's analysis of individual attitudes and actions. Take, for example, Leopold's insistence on Belgian neutrality in the 1930's. Binion finds Leopold's policy inexplicable on rational grounds. He then brings forward two possible psychoanalytic explanations. The first is that Leopold was imitating his father's stance of neutrality prior to World War I, an ambivalent act of posthumous identification and obedience with a decidedly oedipal cast. The second is that Leopold had been traumatized by the death of his beloved wife, in an automobile accident he had caused, and that he was re-enacting this personal tragedy as a national tragedy. Binion opts for the second explanation over the first. He contends that Leopold's neutralist position did not correspond precisely to his father's, despite his claims that he was following his father's example, and that his policy choices do fit a model of a compulsion to repeat a traumatic event.

Binion argues convincingly that the death of Leopold's wife greatly influenced his political actions. But Binion's own presentation of his materials makes it obvious that Leopold's ambivalent identification with his father set the stage on which his marital tragedy could be re-enacted as national policy. To put it another way, Leopold's neutralism is not explicable on the basis of one *or* the other of these two possible causes, but only through their conjunction. Even if we allow Binion the use of Occam's razor, which he claims to be employing in his choice of one explanation over the other, he does not have a legitimate basis for rejecting the oedipal dimension of this argument. It is obvious, indeed, that he

has an axe to grind with regard to clinical psychoanalysis and that Occam's razor is a disguised version of the more destructive instrument.

Let me say, before turning to one final, critical point, that the major essays in *Soundings* make for very good reading. Binion often finds unexpected and interesting connections within his historical data. He also uses the idea of "traumatic reliving" to excellent effect. It fits with his rejection of clinical psychoanalysis, however, that he does not acknowledge that traumatic reliving is a variation on the theme of repetition compulsion, i.e., that his own pet notion is as clinical and Freudian as the oedipus complex itself.

Up to this point I have contended that Binion takes one step forward in his use of a phenomenological method of psychohistorical research and one step back in his rejection of psychoanalytic concepts which would illuminate his own historical materials. He takes a second step back in his substitution of *"la petite histoire"* for *"la grande histoire"* (pp. 42-44). I will cite a particularly blatant example. The initiating event of World War I was the assassination of Archduke Franz Ferdinand. Binion traces Franz Ferdinand's courting of death at Sarajevo to his relationship to Prince Rudolph and to his ambivalent desire to undo the latter's suicide. Rudolph's suicide is traced back to the death of Mad King Ludwig of Bavaria. This is *la petite histoire.* Binion then concludes that if Franz Ferdinand had not "courted his fate, our world would be incalculably different" (p. 85). In other words, he views the assassination of Franz Ferdinand as the immediate cause and the death of Mad Ludwig as the ultimate cause of World War I, with Rudolph's suicide as the intervening variable. So much for *la grande histoire.* But can his argument really be taken seriously? Binion cannot take refuge behind Occam's razor this time because, in constructing a chain of causes back to Ludwig, he is not using it himself. Moreover, he only takes into account one possible motive of one actor in the historical drama. He arbitrarily ignores the Archduke's other possible motives, the motives of other actors, and the historical stage on which the drama takes place. He denies the importance of other causes (economic competition, for example), yet he must provide a narrative of events that is structured by those larger historical causes in order to get on with the story. Methodologically, therefore, he emerges as the Humpty Dumpty of psychohistory. In *Through the*

Looking Glass Humpty Dumpty tells Alice, "When I use a word . . . it means just what I choose it to mean—neither more nor less." In *Soundings* Binion seems to be telling us, "When I explain an event, it means what I choose it to mean—neither more nor less."

The a priori limiting of the meaning of events is, of course, a way of dictating their meaning. When all is said and done, Binion is guilty of the same dogmatism of which he accuses the clinicians. It is a case of the pot calling the kettle black.

EUGENE VICTOR WOLFENSTEIN (LOS ANGELES)

THE LITERARY USE OF THE PSYCHOANALYTIC PROCESS. By Meredith Anne Skura. New Haven/London: Yale University Press, 1981. 280 pp.

Psychoanalytic studies of literature generally suffer by virtue of their interdisciplinary nature. Justifiably unfamiliar with what might be called "clinical" in the practice of literary criticism, psychoanalysts have no familiarity with a body of critical conventions which wishes to protect the whole text, including its surface. As a consequence, the analyst is at best puzzled by the reproach of "reductive" which a critic might level at an analyst's identification of an unconscious fantasy not apparent on the text's manifest level. Conversely, literary critics, lacking actual clinical experience, tend to know and apply only the psychoanalysis that is available in theoretical writings. The result frequently appears reductive to analysts familiar with the subtleties of clinical practice. What occurs is that the two groups of people are only apparently speaking the same language. In fact, an apparently common vocabulary refers to concepts which have evolved from very different sets of experiences. The critic does not appreciate the possible function of identification of latent fantasy content, and the analyst does not comprehend the critic's very different concept of interpretation. Given this situation, it is not surprising to find that misunderstanding has taken the form of disagreement and mutual disregard.

Meredith Skura's *The Literary Use of the Psychoanalytic Process* should go a long way toward clarifying misunderstandings and fostering dialogue. Informed by a clinical knowledge of psychoanalysis and a practical knowledge of literary criticism, it offers both the critic and the analyst a perspective on the other's endeavors.

The author manages the impressive feat of remaining almost equidistant from the two perspectives, so that readers of both disciplines can be reoriented rather than disoriented.

Skura accomplishes this by shifting the debate away from the question of the relative value of id content. Instead, she emphasizes the simultaneous presence of *differing* modes of representation present in the literary work. These range from the primitive and unconscious to the conscious and highly crafted, and they reach different levels of awareness in the reader. The emphasis on differing modes of representation allows Skura to avoid confusing Freud's theoretical insights with the poets' imaginative ones. Viewed from the standpoint of modes of representation, the poets and Freud are equals, each engaged in different attempts to weave primitive elements of the human psyche into the fabric of conscious existence. The constructions of psychoanalytic theory and of poetic imagination emerge as varying representations of similar affective experience.

If, as Skura believes, the effort to represent unconscious elements in conscious terms may be said to define psychoanalysis, then it may also be said that the poets preceded Freud not only in the discovery of the unconscious, but also in the discovery of psychoanalysis itself. This definition of psychoanalysis is, of course, one which focuses very broadly on method. It leaves aside a view of psychoanalysis as a body of theoretical knowledge that enables material to be organized, technique to be elaborated, and patients to be helped. It might easily be argued that every conscious representation from symptom to Supreme Court argument to flimsy psychologizing weaves primitive and conscious elements into its whole, so that Skura's definition, if taken too far, might appear as a case of psychoanalytic insight being used to negate itself. But such an objection would be off target. In practice, Skura's position is extremely valuable. What she does is to insist on an approach that recognizes that the text is not a patient but a representation, and that psychoanalysis is not a reified theory but a subtle process. Through her reorientation, she demonstrates that there has been reduction on both sides. To counter this trend, she organizes the corpus of psychoanalytic approaches to literature into five different models, each an aspect of the clinical exchange, and develops them according to her own useful, clinical approach.

The models she defines, each in its own chapter, concern literature as case history (content), as fantasy (psychic function), as dream (mode of representation), as transference (rhetorical exchange), and as psychoanalytic process, a model which subsumes the others. She concentrates entirely on approaches to the text itself, omitting biographical studies of authors, psychodynamic explorations of individual readers, and comparisons between Freud's philosophy and that of the poets. The five models have a chronological significance that mirrors Freud's personal movement away from the referential significance of his patients' discourse to its psychodynamic function, its mode of representation, and finally its interpsychic function. The application of each model to a literary text shows a related movement away from the realistic surface. Of great importance here is the distinction Skura makes between this movement away from the surface and the stripping away of manifest content, familiar especially in biographical studies, which is the kind of reduction most antipathetic to critics. For her, there is no sharp cleavage between manifest and latent content; there is, instead, a more subtle and gradual transformation from the modes of representation which are most consciously available to those which are more primitive and less easily apprehended. Skura insists on the importance of carefully defining this relationship within the text so that we can come to understand the way in which what is more primitive and less easily visible within it influences our perception of the whole. To do so, we need to know where to look for latent meaning and how to use it to illuminate the text when we find it.

These dual concerns characterize Skura's scrupulous approach. In the chapter on literature as case history, for instance, she argues that one place *not* to look for latent content is in the "unconscious" motives of a literary character. The literary character is part of a larger figurative expression of human behavior, in which irrationality "does not include the kind of unconscious experience upon which analysis is based" (p. 38). When the poet represents irrational behavior in his imaginary universe, it is to make sense of that behavior, much as the theoretician does in an article. Hamlet does not only think Denmark is rotten; Denmark *is* rotten. Hamlet's indecision speaks about his world as well as about his character. Believing for these reasons that it is better to look at the whole play than to the character's "unconscious motives," Skura describes the

oedipal conflict that Freud found in *Hamlet* in a somewhat different way: Hamlet's world "recreates every adolescent's fantasy about growing up" (p. 47). But it is the fantasy which is recreated, she believes, not the fantasizer.

Skura's insistence that literature represents universal affective experiences in ways that are different from the way in which psychoanalytic theory represents them enables her to draw a crucial and very necessary distinction: with its recreation of intense and conflicting passions, *Hamlet* depicts the *experience* of the oedipal child much more accurately than does *Oedipus Rex*. Sophocles' representation is closer to Freud's theoretical representation than it is to the actual childhood experience. What we must seek out in scrutinizing literary characters, Skura concludes, is the difference between the fictional experience the characters enact and the affective experience in our lives to which the whole action refers.

The chapters on fantasy, dream, and transference continue and even surpass the high level of the one on case history, clarifying the misconceptions of each discipline and suggesting theoretical points of view which allow latent content its relevance in the apprehension of a work of art. It is in the discussion of the fifth model, that of the psychoanalytic process itself, that Skura's position is most painstakingly elaborated. To illustrate the manner in which psychoanalysis allows us to understand the text in several different ways simultaneously, she turns her attention to *Measure for Measure*, that most problematic of Shakespeare's plays. Her exploration moves steadily down from the surface without ever losing sight of it. She proceeds more clinically than literarily, pointing out unnoticed details and contradictions, then solving these contradictions, deemphasizing certain themes and delineating less readily apparent ones along the way. She convincingly demonstrates how psychoanalysis can reorient our experience of the text in the same way that psychoanalysis reorganizes a patient's experience of himself and his world.

Any book which takes so general an overview of two disciplines must, of necessity, repeat ground familiar to experts in each. Analysts may therefore find explanations that are occasionally too familiar; they will also, however, find much that is not. Given the breadth of the book, there is unfortunately little opportunity to explore issues of psychoanalytic theory. I would have welcomed

discussion of Skura's theoretical assumptions concerning the conceptualization of the text and the relationship of manifest to latent content in the text, for instance. I might also raise specific questions about method. Indeed, this book should stimulate informed dialogue in these areas, as well as in several others. But to take issue here with a particular theoretical formulation would be nit-picking. The importance of the book is that it constitutes the ablest, most informed, and most complex synthesis of psychoanalysis and literary criticism that we have. Too full of insight for easy summary, it is required reading for all who are interested in the relationship between psychoanalysis and literature.

GAIL S. REED (NEW YORK)

LIVES, EVENTS, AND OTHER PLAYERS. DIRECTIONS IN PSYCHOBIOGRAPHY. Volume IV, Downstate Psychoanalytic Institute Twenty-Fifth Anniversary Series. Edited by Joseph T. Coltrera, M.D. New York/London: Jason Aronson, Inc., 1981. 320 pp.

This volume in the Twenty-Fifth Anniversary Series of the Downstate Psychoanalytic Institute is divided into four sections. The first is devoted to an introduction by the editor. The three subsequent sections contain contributions by an outstanding group of writers in the field of psychoanalytic biography.

Coltrera's introduction is a rich and scholarly survey of the historical, philosophical, and scientific origins of psychoanalytic biography. Beginning with Freud's roots in nineteenth century German romanticism and ending with the political psychobiography of the nineteen sixties and seventies, the editor attempts to blend the approaches and methodologies of psychoanalytic biography with history, philosophy, and the development of psychoanalytic theory. It is an ambitious undertaking, for which Coltrera seems uniquely qualified, but it deserves a book in its own right. As an introduction it is complex and sometimes rambling and disorganized. While touching upon the contributions that are to follow, it does little to establish a framework for them.

In the course of his survey, Coltrera highlights a number of the methodological problems in psychoanalytic biography: the scarcity of primary psychoanalytic data, reliance on analogues of free association (letters, notes, diaries), and the lack of checks on counter-

transference (i.e., what motivates the selection of a subject). Most contributions are heavily weighted toward the libidinal side, with a corresponding lack of developmental and ego psychological considerations. He considers the vicissitudes of aggression in the lives of creative people to be a particularly important and often neglected area.

It is difficult to glean any particular conclusions from the editor's introduction—and perhaps this is intentional. Regarding the "Directions in Psychobiography," they seem to have paralleled those of psychoanalytic theory in general, in that they have tended to stress new developments in theory (identity, object relations, narcissism).

Section Two, subtitled "A Walk in a Darker Wood," contains studies of two creative artists whose lives ended in suicide: Yukio Mishima and Sylvia Plath. Sidney Tarachow's paper on Judas and Christ, the only one in the book dealing with "historical" figures, provides an excellent introduction to the other two papers.

In "Judas, the Beloved Executioner," Tarachow calls attention to the libidinal aspects of the Judas-Christ relationship which forms the core of Christian anti-Semitism. Christ, the willing sacrificial victim, out of his love for the Jews, indicates Tarachow, selected Judas, his favorite, to be the one (in a sense) to kill and eat him. Other anthropological and clinical data are presented to further illustrate the theme of killing and eating in the service of love—an archaic form of love in which killer and victim are fused. The Christ-Judas relationship is seen as the mythical parallel of a deep, universal, human fantasy (the oral triad). This study has important clinical application in problems involving oral aggression, ambivalence, and instinct fusion.

In "Pyromania and the Primal Scene," Jacob A. Arlow comments on the life and work of Yukio Mishima, the brilliant Japanese author whose life ended in a dramatic, ritual suicide. The repetitive primal scene derivatives in Mishima's writings blend together feelings of humiliation, destructive rage toward the parents, and symbolic representations of fire (condensing libidinal excitement and destructive urges). Another major trend in his fantasy productions was a preoccupation with crucifixion, murder, theater, binding, and ritualized cannibalism. After Mishima disemboweled himself, his favorite disciple and homosexual lover became the "beloved executioner" who decapitated him. This spectacular public suicide-

murder was an acting out of both his primal scene and his sadomasochistic fantasies.

Arlow leaves us with the question of how and why the primal scene (a common experience in the crowded living conditions in Japan) came to have such a pathogenic effect on Mishima's life. I would also like to hear Arlow comment on the relationship between pyromania and creativity.

"Fusion with the Victim and Suicide," Shelley Orgel's penetrating study of Sylvia Plath, is an example of the mechanism delineated by Tarachow. This highly sensitive poet struggled with two sets of identifications: one with a chronically ill, then dead, oedipal father and another derived from a more primitive wish to fuse with the preoedipal mother. Her creative efforts are seen as a desperate attempt to save herself from this deadly fusion and to make life possible by externalizing her aggression and recreating object relationships. The attempt eventually failed, and she killed herself.

Orgel focuses on the paradoxical hazard facing creative artists. The same factors which contribute to their creative genius also can release aggressive energies which can destroy them. This becomes a particular danger in the case of artists who, like Sylvia Plath, "cathect their works predominantly with primitive aggression" (p. 137). (How and why some do this remains a question.) For such artists, the creative process becomes like the expulsion of a malignant part of themselves.

An interesting variety of subjects comprise the third section, which is subtitled "The Childhood of the Artist." The paper by William G. Niederland, "An Analytic Inquiry into the Life and Work of Heinrich Schliemann," examines the famous nineteenth-century explorer and archaeologist who discovered the ancient site of Troy. Niederland directs his attention to three aspects of Schliemann's life: his intricate personality development; the unconscious motives behind his abrupt change from big business to archaeology; and the factors stimulating his creativity. His preoccupation with death and his obsession with uncovering the buried past are convincingly related to longings for his dead mother and identification with his dead brother (of the same name).

Niederland documents, with Schliemann, the characteristics generally found in creative people: a particular intensity and durability of sensory experiences, persistence of the family romance, nonrelin-

quishment of the oedipal object, and a perseverance of restoration attempts with regard to lost objects. Regarding the relationship between creativity and psychopathology, Niederland agrees with Eissler that "psychopathology is indispensable to the highest achievements of certain kinds" (p. 198).

In his study of the early life and work of Rudyard Kipling, Leonard Shengold defines "soul murder" as deliberately destroying the individuality of another person—an act most readily carried out against children. For his first six years of life in India, Kipling was indulged by his parents and his ever-present nanny. Then, suddenly, without explanation, he was sent to live with a family in England for the next six years. His guardian proved to be a sadistic, torturing woman who made his life miserable. Shengold describes how Kipling used his creative writing to salvage his soul during the agonizing years in this "house of desolation." He became a great artist; yet the effects of the attempted soul murder, and his intense rage toward his mother for abandoning him, permanently inhibited his capacity to feel joy and love.

The last two papers in this section deal with the adoption theme in the works of Edward Albee, who was himself adopted at the age of two weeks. Jules Glenn's paper studies this theme in its disguised form in *Tiny Alice* and in its open portrayal in *The American Dream*. Glenn outlines the confusions, fantasies, and anxieties of adopted children and demonstrates how these are worked over in Albee's plays. In contrast to the pathologic aspect, Glenn emphasizes the adaptive application of Albee's conflicts in his creative writing.

In "A Psychoanalytic View of Who's Afraid of Virginia Woolf," Harold P. Blum traces the adoption theme in this popular, emotionally wrenching play. The two couples in the play form two sets of childless parents, analogous to the two sets of parents of an adopted child. The secretive, teasing, sadistic games they engage in are viewed as expressions of the identity confusion, alienation, disillusionment, and rage that characterize the family romance fantasy of the adopted child.

Glenn and Blum agree as to the important idiosyncratic elements that comprise and accentuate the family romance of the adopted child: the disturbed sense of reality at having "real and false" parents, the heightened curiosity and confusion as to biologic

origins, and the rage at the original parents for giving them up. It is curious that Blum in his study makes no direct mention of the fact that Albee is an adopted child.

Part Four, subtitled "A Holy Sinner," consists of three studies of Dostoyevsky by Mark Kanzer. Because of his remarkable perception into the deepest of emotional conflicts, his severe psychopathology, and the clearly autobiographical nature of his writing, Dostoyevsky has been a favorite subject for psychoanalytic biography beginning with Freud. Through the many characters in his novels, he reveals himself as a seclusive, brooding hero obsessed with incestuous wishes and parricidal urges. Kanzer describes the intricate series of maneuvers through which Dostoyevsky strives to reconcile his conflicts: by escaping into hallucinatory dreams and fantasies in which he attempts to resurrect the good, split-off images of his parents; by turning his murderous impulses toward himself (as suggested by his epileptic seizures which began after his father's actual murder); and by the phases of religious and political conversion through which he attempted to reinforce his shaky superego.

Dostoyevsky is one of that company of creative geniuses whose artistic works vividly document their lifelong struggle against emotional illness. He suffered from severe claustrophobia and epilepsy (possibly psychogenic) and exhibited schizoid and paranoid trends. Kanzer points to his father's psychosis and speculates that Dostoyevsky might have been schizophrenic. We are faced once again with that tantalizing question of the relationship between psychopathology and creativity. Kanzer mentions (but does not pursue) the correlation between Dostoyevsky's creative urges and his convulsive seizures, which increased markedly during his periods of literary productivity.

If the vicissitudes of aggression in the lives of creative people have been neglected in the field of psychobiography, as Coltrera states in his introduction, this neglect is not evidenced in the examples he has drawn together in this volume. There are few books written on this subject, and even fewer have something worthwhile to contribute. As a collection of outstanding examples of psychoanalytic biography and a discussion of the theory of creativity, this is a most valuable and enjoyable book.

LEON E. A. BERMAN (BIRMINGHAM, MICH.)

VERBAL BEHAVIOR. ADAPTATION AND PSYCHOPATHOLOGY. By Walter Weintraub, M.D. New York: Springer Publishing Company, 1981. 214 pp.

The author's principal hypothesis is that the choice of syntactic structures in speech and the choice of defense are correlated. It follows that personality traits and psychopathological syndromes, to the extent that these are associated with specific defenses, are also correlated with different patterns of syntactic usage in speech.

The author's method is, generally, to analyze samples of speech (monologues elicited by a standardized procedure) to obtain the frequencies of occurrence of members of fourteen categories (not all of which are, strictly speaking, "syntactic"): the quantity of speech; long pauses; the rate of speech; nonpersonal references; I; we; me; negatives; qualifiers; retractors; direct references; explainers; expressions of feeling; and evaluators. Then groups representing "no pathology" and various psychopathological syndromes are compared with respect to the frequencies with which these categories appear in speech.

The author notes that psychopathological syndromes tend themselves to be defined in terms of linguistic behavior, so that correlations between types of linguistic behavior and types of psychopathology may express a kind of tautology. Therefore, he makes an effort to select members of his "deviant behavior" groups on the basis of nonverbal criteria. How successful he is, the reader must judge.

In addition to different kinds of deviant behavior, different age groups (children, adolescents, adults) are compared in terms of the same categories. These categories are also used in one chapter, in "a new approach to psychohistorical research," to analyze the Watergate transcripts and to write personality profiles of the participants. In another chapter, they are used to compare samples of spoken and written language. In a third, they are used as part of an attempt to investigate the verbal communication of affect by analyzing samples of "angry speech" collected from actors paid to simulate an angry state.

The data in and of themselves are interesting. They reflect a commitment to the use of empirical methods, rather than merely reflection, to study the role of using and responding to different characteristics of speech in clinical communication. This book is

recommended to those individuals (increasing in number) who see the latter as an important research problem in psychoanalysis.

However, a warning is in order. The author tends, perhaps in an excess of enthusiasm, to overinterpret the data. The group differences that are discovered are "explained" *ad hoc,* with little or no attempt made to consider alternative interpretations of the findings (which are frequently unpredicted or contrary to what had been predicted). It follows then that insufficient thought is given to the evidence required for choosing one interpretation of the findings over another.

Finally, the question of the way in which defenses may find expression in different patterns of speech needs further conceptual explication. It is not enough merely to assert on the level of common sense that a particular defense is surely connected with one or another of a rather mixed bag of verbal and paraverbal categories. Such an approach can lead to oversimplification and questionable conclusions.

MARSHALL EDELSON (NEW HAVEN)

WORKING THROUGH NARCISSISM. TREATING ITS SADOMASOCHISTIC STRUCTURE. By Maria Carmen Gear, M.D., Melvyn A. Hill, M.D., and Ernesto Cesar Liendo, M.D. New York/London: Jason Aronson, Inc., 1981. 422 pp.

This book expounds the thesis that all mentally sick persons can be categorized either as "patients," those who suffer the anxiety of mental illness, or as "agents," those who reinforce such anxiety or transmit it to others. Taking power considerations as a main focus, the authors classify all analysands as fitting into four sadomasochistic groups: the sadistic agent ("sadist"), the sadistic patient ("borderlined"), the masochistic agent ("borderliner"), and the masochistic patient ("masochist"). In their introduction, the authors promise a discussion of some potentially fascinating material. This includes a semiotic model of the psychic apparatus; a demonstration of how semiotics permits a systematic analysis of the analysand's speech, actions, and affects; the development of a general psychoanalytic theory based on the sadomasochistic patient-agent dichotomy; an integration of the social and political dimensions of

sadomasochism with the aims of analytic treatment; and a redefinition of the classical recommendations for analytic treatment.

These are heady promises, but the authors are unable to live up to them. Indeed, the book's defects are so numerous and severe that I cannot detail them. I will, instead, give a few examples which I think will convey some of the problems.

First, the authors' theoretical approach consists of an amalgamation of Kleinian theory and the French School, but in a terribly oversimplified way. They state, for example, that there is a binary structure to the psychic apparatus of narcissistic patients: "It consists of the various characteristics divided into two fundamental characters in the behavior and conceptualization of each subject: the ego and the alter. Similarly, all the affective states of the actual ego and alter are reduced to two fundamental ones: satisfaction and frustration. All actions are reduced to 'to satisfy oneself by frustrating others' and 'to be frustrated while satisfying others.' All qualifications are reduced to bad and good" (p. 108). And the authors follow this closely throughout the book. The entire focus of the book is on the sadomasochistic patient-agent who is good or bad, frustrated or frustrating, satisfied or satisfier, or has various permutations thereof. One can turn to any page of the book to find various combinations of these descriptive terms. Often one finds very little else.

Second, the authors are idiosyncratic to the point of confusion. The tendency toward inventing new words is already apparent in the terms "borderlined" and "borderliner." Further examples ("para-counter-acting-out," "alterectomy," "outsight," "feared mnemic trace," etc.) can be found in every chapter. Familiar concepts are redefined (e.g., "what is traditionally called the ideal-ego we now call the superego, and what is traditionally called the ego ideal and the superego we now call the superalter" [p. 67]), and sweeping statements are casually made (e.g., "we were able to study the psychopathological disturbances which resulted from the three main sickness-producing factors: censorship, dependency, and blackmail" [p. 18]).

Third, the authors' recommendations on technique confirm the impression that they are speaking of something other than psychoanalysis as most of us know it. Take, for example, the following: "the analyst should requalify [the masochistic analysand] as good,

clever, and sensible, or, at least, as not bad, not stupid, not crazy. He should also stop the analysand from using the descriptions, reconstructions, and interpretations to disqualify himself again and again" (p. 370), or "the analyst will next be in a position to give the analysand some clear therapeutic directions about how to avoid repeating or being locked into his old compulsive repetitive pattern" (p. 372).

In brief, the book focuses on a narrow and very idiosyncratic view of the sadomasochistic aspects of narcissistic disorders. It is unlikely that its contents will be met with much agreement or acceptance by psychoanalytic readers. One hopes that the book represents preliminary impressions to which the authors will give further reflection and reconsideration.

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ABSTRACTS

Revue Française de Psychanalyse. XLIII, 1979.

Abstracted by Emmett Wilson, Jr.

Anxiety Denied. François Pasche. Pp. 5-17.

Continuing his reflections on the distinction between neurotic anxiety and psychotic anxiety, Pasche discusses the primary modes of defense against psychotic anxiety. In his important 1953 paper, "Anxiety and the Freudian Theory of Instincts," Pasche had made anxiety the fundamental experience of the self. Under the influence of Kierkegaard, he wrote of pure anxiety as the anxiety of nothingness and drew the conclusion that such anxiety implied the constitution of an object and a space between the self and the object. In the present paper he reviews this earlier position, focusing on the more complex phenomenon of psychotic anxiety, which involves the loss of one's being through absorption into the other or through the invasion of the self by the other. Some writers have discussed the so-called perverse defense against schizophrenia, i.e., the projection of the conflict onto the other. Pasche discusses projections into the transitional space, which, if successful, may become sublimations. His particular concern is with philosophy. He classifies philosophers into two groups: the oedipal philosophers, who accept reality, contradiction, and contingency and are thus philosophers of anxiety; and the system builders, who subordinate reality to a compulsive need to unify their knowledge and understanding and whose conflicts are preoedipal. Pasche singles out Spinoza, who, he claims, rejected the image of the father; this led to a guilt that drove him to deny freedom as well as anxiety. The result was a regression so severe that, to avoid dissolution of the self, a projection of the conflict was instituted. In other words, a philosophical system was constructed to avoid psychotic anxiety.

Communicated Anxiety, Communicating Anxiety. Jean Gillibert. Pp. 19-29.

Gillibert focuses on the function of anxiety within a system of communication between two individuals. He argues that anxiety is the fundamental, schematic, and existential link in each psychic couple, the existential link by which a couple remains a couple. He illustrates his thesis with clinical material concerning the communication of anxiety between patient and analyst, and with a literary example, the communication of anxiety between Macbeth and Lady Macbeth. Anxiety is linked to impossible representations of death, to a break in narcissistic continuity. Even Kierkegaard's own experience of anxiety in the rupture of his relationship with Regina Olsen must be viewed analytically as a source of his philosophical focus on anxiety.

A Time for Anxiety and a Time for Pleasure. Jacques Cain. Pp. 30-44.

Cain emphasizes the indissoluble links between pleasure and anxiety. In the literature we find an opposition between anxiety and pleasure. Anxiety is sometimes presented as the outcome of unsuccessful strivings for pleasure. But, clinically, the two are not that separate. Pleasure, as many theorists have noted, is never complete, and there are situations in which anxiety can become invested with pleasure, as Freud noted in his discussion of the pleasurable effects of heightened tension. Cain also sees a problem in the distinction between the fear of a real danger and anxiety before a real danger. Fear becomes anxiety when linked to ambivalence, i.e., attraction, as in the fear of heights. Cain considers three clinical examples of this link between pleasure and anxiety in humor, in hysteria, and in masochism. He concludes with a discussion of the various mechanisms by which this intrinsic link between anxiety and pleasure can take place.

Anxiety and Narcissism. André Green. Pp. 45-87.

In this closely argued paper on the interrelationship of anxiety and narcissism, the author criticizes Freud's discussion in *Inhibitions, Symptoms and Anxiety* because the question of primary narcissism of the neonate is not well worked out. It is difficult to see how the fetus moves from an absolutely narcissistic position, in which it does not know the mother at all, to the ego of the oedipal phase. Green suggests that Freud was correct to reject Rank's hypothesis of birth trauma, for, as Winnicott showed with his concept of holding, there occurs after birth a reconstitution of conditions as close as possible to intrauterine life, amounting to the external nesting of the infant. A second birth, the loss of the breast, is really the first or psychological birth of the ego, assuring its distinctness from the object. Green argues against phenomenological concepts of the ego, which neglect the purely metapsychological character of the ego. He maintains a certain reserve with respect to such terms as "consciousness of the self" or "identity," which are phenomenological in their inspiration. There can be no ego representation, for the ego is a theoretical concept, not a phenomenological description. The ego deals with representations, but is not itself represented. When one speaks of narcissistic relations, what Winnicott termed ego-relatedness, we must realize that this is used loosely. Ego representations are in fact representations of the object which are then changed into representations of the ego by narcissistic cathexes. Only thus can one begin to talk intelligibly about narcissistic anxiety. Phenomenologically, it is appropriate to describe such manifestations, but, theoretically, narcissistic anxiety is anxiety about objects transformed into narcissistic objects. Thus the ego functions as a reservoir of operations without any representation of itself. One of these operations is identification, which suppresses the distance separating object and ego. Absence of an object interrupts the ego's fragile success in maintaining its investments at a relatively constant level. Green develops the concept of object trauma. Since the object is inconstant and not under the control of the ego, the ego is forced into modification of itself. A narcissistic solution develops by an augmentation of ego cathexes,

but this eventually makes the ego even more vulnerable, leading to depression or to projective identification with feelings of persecution by the object. Such narcissistic responses expose the ego to threatening narcissistic anxieties. Green distinguishes narcissistic anxieties from psychotic anxieties which develop when the primary object does not fulfill its role as a mirror, an ego auxiliary, but becomes a source of conflict. The ego must then defend itself on two fronts, invoking reserves from destructive impulses directed toward the external object and onto the self in turn. The projective identification will then become excessive, as the object trauma becomes an object of psychosis, with a destructive character for psychic reality. Green notes that there has been a remarkable convergence in the clinical and theoretical recognition of the importance of narcissistic impulses, in spite of the differences among particular theorists.

Catastrophic Anxiety: New Aspects of the Psychoanalytic Theory of Anxiety. Jean Begoin. Pp. 89-95.

Begoin views anxiety as a primary affect manifested in different modes according to the level of psychic functioning. In reviewing Freud and Klein on the primary form of anxiety, he feels that some modifications of the theory are in order. Until recently the primary form of anxiety seemed to be paranoid anxiety linked to setting in motion the very precocious mechanisms of splitting and idealization of the object and the self. Work with autistic children, however, suggests a phase anterior to splitting and idealization. Begoin argues that the most primitive form of anxiety is catastrophic anxiety. The feared catastrophe is the failure of a state of total dependence, being torn away from the object, separated as if one tried to separate the front and back sides of a piece of paper. This threat of the loss of narcissistic identification is experienced as a catastrophe, in the sense of a total chaos of the infantile unorganized self.

Anxiety as Identification. Its "Go-Between" or Messenger Function in Treatment. René Berouti. Pp. 97-115.

Berouti discusses the place of anxiety in psychoanalytic sessions. His hypothesis is that anxiety is an archaic mode of thought, a mode of perceiving and sensing. It takes on a communicative force from the resonance of affect which it raises in the analyst from a primordial identification with his patient. These mnemic traces of primary cathexes in the experience with the primary object underlie and maintain for the patient and for the analyst the identifications and fantasies of the primal scene. Berouti thus views anxiety as a singular mode of psychic liaison.

Anxiety, One and Indivisible. Jean Chambon. Pp. 133-141.

Chambon argues for the unitary character of anxiety, whether its manifestations attach to genital fantasies in the course of a neurosis or express pregenital fantasy life in psychosis. Anxiety is all of a kind in its consequences at the level of the ego. In treatment one sees parallels in the modalities of expression of

anxiety whether the instinctual process be sexual-aggressive or self-preservative (narcissistic). Chambon believes that there should be further metapsychological studies of anxiety. Even though the bibliography on anxiety is huge, not much new has been contributed since *Inhibitions, Symptoms and Anxiety*. Pasche's work on anxiety is of great importance, offering a very deep psychoanalytic study of anxiety. But the claim for an essential duality is not, Chambon argues, established.

Metaphysical Anxiety and Castration Anxiety. Colette Chiland. Pp. 143-148.

Chiland criticizes Pasche's discussion of Spinoza, for she does not see how terms like oedipal or pregenital can apply to the great philosophers. Philosophy and science were born at the same time. They are two regions of discourse, one of which, science, has become very successful. Philosophy has been less successful, but we cannot expect man to renounce discussing the self, the world, God, time, liberty, etc. Chiland sees creativity as a particular form of struggle against anxiety, whether the work created is philosophical, literary, artistic, or scientific. The work is something more than a direct discharge of impulses, a direct expression of anxiety. Our encounter with such cultural constructions overshadows the narrow psychoanalytic reading of the work, and only a few literary texts approach free association. We must understand that metapsychology is not metaphysics, and Freud by no means dealt with the metaphysical question of anxiety.

Anxiety and Repression. Jacqueline Cosnier. Pp. 153-159.

It is more consistent with Freud's thought in the structural hypothesis to renounce a causal relationship between anxiety and repression. The question of whether anxiety is the cause or the result of repression is an ambiguous one. Repression unlinks word and thing representations, which induces a primary process condensation of affects and tends to create anxiety. At the same time, anxiety mobilizes the defense mechanisms which are a part of repression. Cosnier illustrates her arguments with a personal example of anxiety arising from a choice imposed by necessity. She does not find tenable the distinction that Pasche would have us make between neurotic and psychotic anxiety. Rather than make a radical distinction, we should deal more with the means of elaboration of the psychic apparatus. Such elaborations are means for recovering, at any cost, the feeling of personal unity. Phobias, projections, compromise formation with symptoms, inhibitions, creativity, are all means to achieve the same end.

Concerning an Uncertain Anxiety. Claude Le Guen. Pp. 191-194.

Le Guen, like Chiland, argues against the classification of philosophers into oedipal and nonoedipal, both in principle and in the case of Spinoza cited by Pasche. According to Le Guen, psychoanalysis has no right and no privilege to make moral judgments even in Freudian terms on what is not within its specific field. All that is human is not in this field, and there are many risks in overstepping the restricted area of psychoanalytic appropriateness. Le Guen argues

against Pasche's discussion of anxiety and examines Freud's concept of anxiety to show inconsistencies in Pasche's argument.

Transmission or Process. Robert Barande. Pp. 211-221.

The term "transmission" has been used since 1967 in the Parisian Psychoanalytic Society as a substitute for the expression "didactic analysis." Barande has criticized this term as too much of a synonym for didactic, in that it objectifies and reifies the process around two static poles, the transmitting analyst and the receiving analysand. For him, "didactic" and "transmission" equally misrepresent the singular experience of the discovery of the psychoanalytic process. He suggests that the introduction of the phenomenological concept of transmission into metapsychology runs the risk of making the transmission transference operate as a means of identification with the analyst. He notes wryly that the German term for transmission is the same as that for transference, *Übertragung*. Further confusions and disastrous consequences arise when transmission is made to stand for the psychoanalytic process itself. Not only have we failed to provide a theoretical underpinning for didactic analyses, but we now find transmission set up as the heart of every psychoanalytic process. For Barande, the concepts of transmission and cure are outside the analytic process and apply to it only after the fact; that is, they are phenomenological, not metapsychological concepts. Introducing into the psychoanalytic process a goal, whether it be cure or transmission, can only lead to an impasse, an interminable analysis, in the face of the analyst's countertransference.

The Transmission of Psychoanalysis. Jacqueline Cosnier. Pp. 227-238.

Cosnier discusses three topics in the transmission of psychoanalysis: what transmission is; what is transmitted; and the desire to transmit. It is uncertain what transmission is, she claims. It is banal to say that psychoanalysis cannot be transmitted like other sciences, that it requires the singularity of an experience. The paradox of transmission is the same as the paradox of that which is discovered; it is a paradox in Winnicott's sense of the created-found. It has become especially important to recognize this since the recent work by Viderman on the transitional space of analysis and the creative aspects of treatment. Some of the limits of transmissibility can be discerned. The personal background of each analyst limits what comes to him from experience and what he can use of his experience, of his own personal psychic economy. And, just as analysts show differences in their ability to use their experience, so patients differ in their openness to change. As for what is transmitted, it is the creation of a new system of mental functioning, of intra- and intersubjective perception. Psychoanalysis has opened the world of representations to the plurality of desires, fantasies, identificatory positions, etc., with a displacement of values onto psychic reality as determined genetically through the personal history. There is also a creative remembering, rather than a simple recall. Not just a knowledge of the unconscious is gained, but an experience of it. The analyst tends to transmit his own means of elaboration and to transmit different things at different times,

for his experience, too, is continually enriched. The desire for transmission involves the desire for procreation and offspring, certainly, but it involves as well the uniqueness of the experience of the analysis for the analyst himself. Each analysis has its moments of affective richness, in which the analyst re-experiences his own proper existence.

The Analysis Called Didactic. Jean Cournut. Pp. 239-246.

There is a manifest and a latent content when an individual presents himself for analysis. The analyst has discovered this distinction in his own analysis and relives it daily in his analytic hours. There is a need to discern these contents early in the process. If we take a theoretically rigorous position, there are only two tenable positions in analysis: either a patient comes into analysis to analyze the formation of his unconscious in all the diverse facts in which it is disguised, including even the wish to be cured of his neurosis; or we have a patient who comes certified as a candidate through an institute. For Cournut, there is an inherent contradiction built into the psychoanalytic process if the latter is called a didactic analysis. An analysis should be classified as didactic only *a posteriori*. Yet this is made difficult by the titles of training analyst and candidate and by the structure of the system which requires that a didactic analysis be undertaken, rather than an analysis. Thus the didactic analysis constitutes an exception to the distinction between manifest and latent requests. One of the reasons for the institutionalization of a didactic analysis was the wish to avoid theoretical heresy, that is, a nonanalytic reason for analysis. We have come to confuse the temporal requirement that an analyst know his own unconscious before undertaking treatment of others with the qualitative notion of a didactic analysis. The irony in this is that analysts who are the "best analyzed," and thus presumably best able to defend analysis against heresies, have received their training through a didactic analysis, which is a sort of practical heresy. This theoretical error has become ingrained in the movement, guaranteeing a sort of elitism in the institutions, with guaranteed honors and guaranteed clientele for training analysts.

Transmission-Resignation. Jean Gillibert. Pp. 247-259.

Psychoanalysis is characterized by a tradition which has been set up with a text, a doctrine, etc. But it is not a fundamental science or a discipline of exactness and specificity. Psychoanalysis oscillates between becoming a dogmatism of a revealed text and a dogmatism of learned ignorance. Since it has become self-referential and sectarian, psychoanalysis has become more and more convoluted in its speculations, has forgotten life and death, and is in the process of losing itself deliberately. For Gillibert there is no transmission except that of a meaning, or the meaning of meaning. There is no transmission in psychoanalysis of a meaning revealed once for all time. The linguistic model of de Saussure, as travestied by Lacan, has been recognized as an illusion. But there is no operative or epistemological model, whether linguistic or biological, which will account for the totality of the doctrinal corpus of psychoanalysis, much less for the process of therapy which goes beyond our knowledge. We can see certain ideas

returning, like the return of the repressed. Such would be the rediscoveries of hypnosis and catharsis. We see an intellectualization developing about Freudian terms and ideas, which are as bandied about as household terms. Yet psychoanalysis still has the power of transmission, and it is due essentially to the therapeutic project itself.

Transmission, or the Death Instinct in Institutional Discourse. Jean Guillaumin. Pp. 261-270.

The subject of transmission today sets certain groups of analysts into opposition. They are suspicious that some masochistic aspects of this stem from the death instinct, and, given the disruptions associated with the question, they may be right. Guillaumin proposes to examine the psychic dimensions of the need to transmit. There is a hesitation to write about the patient's wish to become an analyst and even more hesitation to write about patients who have become members of one's institute. This hesitancy on the part of training analysts to publish on transmission would seem to run counter to an analytic orientation and derives perhaps from fear, even though there should be nothing special about training analyses. Guillaumin feels that the problem of transmission is linked to the identification of analysts with their institutes and is explicable as a group psychological phenomenon. Any group defines its identity by defining the criteria of admission to the group. The reproduction of the group, the maintenance of the group's identity, and the question of the essence of that group are closely related questions. Guillaumin's hypothesis is that there is a sort of collective unconscious involved in the wish to transmit. Many problems in recognizing individual motivations in transmissions come from this group identification, with resulting ambivalence and fantasies of fusion with the good mother. The analyst needs the skill and courage to look at his individual need for transmission, as well as his group identification. The desire to have a child, a pedagogical child, is an important element in transmission, but this also needs to be analyzed. This child, whom the analyst invests in as a future other self, requires that the analyst resolve the ambivalence of his own instinctual demands. Even the postulate that there must be analysis is a defense. If such a postulate is not or cannot be examined, it functions as an idealizing urge for reparative and projective mastery, related to anal ambivalence and to unexamined aspects of the analyst's relation to his own analyst. Sometimes it is his relationship to the institute which is unexamined. The institute's neutrality can also be a screen, whether the analyst declares that the process of transmission is untouchable or that he is against it. In either case, there is the risk that unconscious hatred and projective idealizations may be displaced onto the collective group instead of being analyzed as a personal unconscious aspect of the analyst.

Transmit? What? To Whom? Phillipe Jeammet. Pp. 271-278.

Since a personal analysis is a necessary condition for the training of psychoanalysts, this makes psychoanalysis very different from the preparation required of other scientists. For this requirement of a double role in training (that of

analyst and of analysand) the word "transmission" seems more apt than "education." However, transmission has connotations of being mechanical, leaving out of account the unexpected and spontaneous. It even suggests hereditary transmission, as well as a sort of irrational transmission by faith, or transmission of thought. Perhaps a better comparison would be to the sort of transmission which occurs between artisans as they transmit a skill. Yet we cannot limit transmissions to this, for, as Freud pointed out, psychoanalysis is also an investigative method and a psychological theory. Education is involved in the transmission of the theory just as in the transmission of any body of scientific knowledge. Further, the theory must be confronted with other psychological theory and meet issues of validation. Psychoanalysis is transmitted only to those who practice it, that is, future analysts. But we must recognize that it also belongs to the world. Psychoanalytic organizations should assure a good diffusion of knowledge rather than complain of vulgarization. This is important for the future of psychoanalysis, with its wide-ranging effects on human relations, e.g., in child-rearing, preventive medicine, etc.

Philosophizing about the Training of Analysts. E. Kestemberg. Pp. 287-295.

Kestemberg focuses on how the training analyst functions and how he contributes to the transmission of analytic knowledge. She discusses how analytic training helps an individual acquire the knowledge to function in this capacity. She considers the analyst's therapeutic ambitions, what can be required of him or her in training, how he or she is aided to achieve the abilities, and who is chosen for the function of transmission. She strongly emphasizes the length of training and the requirement that an analyst function in solitude and secret with many different patients who place diverse demands upon him. In spite of this isolation there must be common aspects in the treatment which rest on a theory he has acquired. The training analyst's essential function is to bring to light the unconscious, not to achieve narcissistic fulfillment. (One can sometimes tell who an individual's analyst was by the way the person talks or by what he says, unfortunately.) The training analyst must renounce forcing upon the candidate an analytic ideology and instead find a rhythm appropriate to each candidate. The elaboration of psychoanalytic science within the institutes will aid him in this effort.

An Episode in the Relationship between Freud and the Psychiatry of His Time, as Seen in His Correspondence with Jung. Madeleine Vermorel and Henri Vermorel. Pp. 321-334.

Freud was in contact with psychiatrists in the course of his work, even though he was not himself a psychiatrist. The correspondence with Jung gives us some information on the history of this relationship to psychiatry. Unfortunately, the correspondence with Bleuler is still kept from publication, but we can follow some of the history. Bleuler, though a frequent critic of Freud, defended psychoanalytic concepts when speaking with other psychiatrists. With the publication of the Schreber case, the divergences between Jung, Bleuler, and Freud became

apparent, especially on the concept of psychosis. A rupture was inevitable, but not without constructive results. We may consider Freud's paper on narcissism a response to Jung's *Transformations and Symbolisms of the Libido.* Psychiatry has changed in many ways, but we can now see that the real problem is the question of the methodology appropriate to each field. Psychoanalytic concepts apply to psychoanalytic therapy and are not transposable elsewhere. Psychiatry is a pluridimensional field, with biological, cultural, and social aspects, and it is a part of medicine. It is still mutually beneficial for psychoanalyst and psychiatrist to work together in psychiatric settings, even though their approaches may be contradictory.

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The following abstracts appeared in Psyche and are reprinted with the permission of the editor of the journal.

The Self and the Ego. Rolf Fetscher. Pp. 616-639.

How the concept of the self is related to that of the ego has been discussed ever since Hartmann introduced the concept of self into psychoanalytic theory. It is assumed that the concept of the ego cannot explain all psychic phenomena—especially those falling within the general psychoanalytic psychology of development. On this assumption, the author discusses the relationship between the ego and the self, in the sense that the latter is distinguished by its representational qualities while the former, an organ of the self, is defined solely by its functions.

On the Theory of Psychoanalytic Psychotherapy. Otto F. Kernberg. Pp. 673-702.

Kernberg distinguishes between psychoanalytic-uncovering methods and supportive-covering methods. The latter are employed when the former are contraindicated. Severe character disorders and borderline personalities require a psychoanalytic psychotherapy which differs from "authentic" psychoanalysis as regards the treatment strategy rather than the tactics or technique. The main task is to transform primitive transference reactions (which reflect part-object relations) into typically neurotic ones by means of interpretations. With respect to widespread misconceptions, Kernberg emphasizes that empathy is a presupposition and not a substitute for interpretive work.

The Logical Status of Psychoanalysis. The Problem of Dream Interpretation. Ernst Konrad Specht. Pp. 761-785.

The method of interpreting dreams developed by Freud does not fit into the framework of explanation and prediction which theorists of science have derived from the example of the natural sciences. The method is not arbitrary, however. Its criterion of validity is internal, namely, in the structure of dream narration. Like literary fiction, the dream product goes beyond the conscious intentions of the author. The optimal interpretation therefore refers to the *potential* meaning of the text.

Psyche. XXXVI, 1982.

Preverbal Structures of Logic. Fritz B. Simon. Pp. 139-170.

The logical structures which underline our thoughts and actions develop out of a pre-existing system of communication and relationships. They are expressed in the sign and rule systems which correspond to this communication system, and they are codified in our language (*cf.*, Whorf and Lacan). Simon argues that all logic develops out of the dialectic of the pleasure and reality principles. This includes the logic of psychotic thought, the structure of which he seeks to reveal by reconstructing the preverbal structures of logic in the neonate.

Observations on the Subjective Indications for Psychoanalysis. Josef Dantlgraber. Pp. 193-225.

The concept of subjective indication gives central importance to the reaction of the therapist, that is, to the countertransference, in evaluating the analyzability of a patient in the initial interview. Thus, analyzability is not regarded as a dimension to be measured solely by applying objective criteria to the analysand but must also be assessed in regard to the subjective possibilities of the analyst.

The Silent Analyst. Henning Graf von Schlieffen. Pp. 289-306.

The analyst's silence is not always a sign of his readiness for empathic listening, leaving the active structuring of communication to the analysand. It may just as well indicate an incapacity for responding to the specific requirements of the patient and thus for making the most of the therapeutic opportunities. The author posits that individual drive-defense conflicts or professional ideology, or a mixture of both, are involved.

The Significance of the Dissident for Psychoanalysis. Johannes Cremerius. Pp. 481-514.

Even in Freud's times the discourse of the psychoanalytic community of interpreters and therapists was taken over by forms of thought more appropriate to a religious community (canonization of the correct doctrine, banishment of heterodox or apparently heterodox views, with subsequent splinter groups and section formation). Cremerius demonstrates this with the example of the official theory of psychoanalytic technique which is based on Freud's writings in the years 1912-1915—a scientific stylization of his own "dissident" therapeutic technique. The dogmatization of the "mirror" technique has since prevented the moderation of the relevant rules in the direction of a more patient-centered, situation-specific, and flexible technique such as would be consistent with a dyadic psychology. There has of course been a greater de facto pluralism taking hold in the last forty years within the international psychoanalytic community, with only local dogmatisms surviving. Liberalization of psychoanalytic "essentials" has resulted in a high degree of tolerance. There is no greater consensus today in matters of psychosexual development than there is in questions of metapsychology or technique.

Psychoanalytically Oriented Art and Music Therapy in the Framework of Inpatient Psychotherapy. Paul L. Janssen. Pp. 541-570.

The author presents the possibilities of introducing psychoanalytically oriented art and music therapy into the framework of inpatient psychotherapy. The objective is to utilize the dimension of representational symbolization for the objectivation of self and object concepts and affective conditions. Especially in the case of patients with early disturbances it is possible to strengthen creative ego functions with the aid of plastic-musical "transitional objects."

Mythology of the Imaginary or Imaginary Mythology: History and Critique of the Psychoanalytic Interpretation of Myths. Gunzelin Schmid Noerr. Pp. 577-608.

Myths, such as the Oedipus myth, are akin to an arsenal of traditional models of action and patterns for the elaboration of desires. Historically as well as currently they have a socializing function. They reflect cultural struggles, progress and its price, and the transformations which the profiles of subjects undergo. As a rule, psychoanalytic approaches to myths ignore their historical meaning; that is, the historical difference between the myth and the present. The historical interpretation usually misses the latent, enduring meaning of the myth. What is needed is an interpretive approach to myths which differentiates historically and yet mobilizes the interpreter psychoanalytically. The psychoanalytic interpreter is himself subject to history (and life history) whose potential is constrained by the prevailing reality principle.

The Ascent into the Bourgeoisie and Its Price: The Example of Charles Dickens. Edmund Jacoby. Pp. 609-629.

One can read the work of Charles Dickens (1812-1870) psychoanalytically or psychohistorically as a compromise formation between conscious intentions and unconscious wishes and guilt feelings. According to Jacoby, the contradiction between the conscious and unconscious influences upon Dickens's narrative writings results from the fact that Dickens experienced a precapitalist childhood, that this childhood proved incongruous with his subsequent ascent into the middle class, and that he therefore rejected it as an adult. The depictions of childhood in the novels represent an attempt to design an image of childhood timely for industrial capitalism. But this image did not fit smoothly into the novelist's own childhood experience.

Literature as an Aid to Life: The Example of Thomas Mann. Christian Scherg. Pp. 630-661.

There is a conspicuous affinity between psychoanalysis and the work of Thomas Mann. This is also true in regard to its reception by the reader. Just as there is a model Thomas Mann *hero* in the novels and stories, there is also a model Thomas Mann *reader* who is able to recognize and identify himself with the hero on the basis of his own drive and narcissistic conflicts. The reader can follow the development of the literary figures beginning with the "disgust with

life" in the early works up to the acceptance of inner reality. In re-enacting this development, the reader has the opportunity to constitute a narcissistic equilibrium and a coherent self.

Text Structure and Interpretation. Hermann Argelander. Pp. 700-725.

Psychoanalysis, conceptualized as having to transform unconscious meanings into language, must come to terms with patients' texts having differing formal structure. The author applies an interpretive system to three different texts—a newspaper commentary, a fairy tale, and an excerpt of a novel—in order to disclose the meaning of each as revealed in its formal structure. Argelander asserts that, contrary to a text interpretation, a psychoanalytic interpretation must be able to reformulate a linguistically conveyed text so that described events can be seen as having come about through personally motivated actions and so that the explanation for this can be derived from the life history. This conception clearly converges with the action theory developed by Roy Schafer.

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Meeting of the Psychoanalytic Association of New York

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NOTES

MEETING OF THE PSYCHOANALYTIC ASSOCIATION OF NEW YORK

January 4, 1982. THE SCHEMA: BASIC CONCEPT IN A NON-METAPSYCHOLOGICAL MODEL OF THE MIND. Joseph W. Slap, M.D. and Andrew J. Saykin, M.S.

Mr. Saykin and Dr. Slap offered a model of psychic structure intended to clarify and simplify other formulations that have preceded it, i.e., those that suggest that "experience with reality, including external objects, results in structure building and other changes within the mind." Common to these other formulations are such mechanisms of ego building as: Freud's (and followers') identification, incorporation, introjection, internalization; Nunberg's synthetic function; Kernberg's metabolizing of parental images; and Kohut's transmuting internalizations. The authors hoped that their formulation, which draws heavily on the work of George Klein and Piaget, will encompass these concepts and move toward placing psychoanalysis "within general psychology in distinction from earlier attempts to make psychoanalysis into general psychology." Their basic organizing concept is the *schema*. Borrowing from Bartlett, they defined schemas as active organizations of past reactions or experiences, both perceptual and behavioral, which operate as associated and integrated entities. Organizations of memories, fantasies, affects, and modes of response, such as the "repressed unconscious," meet the definition of schema. Smaller units of the mind would constitute *schemata*. As an example of the latter, they cited Schafer's concept of " 'discrepant suborganizations of the personality' [such as] the fetishist knowing and not knowing the anatomical distinction of the sexes. . . . Schemata can exist as 'antagonistic organizations of motives, mental processes and representations.' " However, regularity of human behavior is possible because a well adapted response is related to "other similar responses which have been serially organized yet which operate . . . as a unitary mass."

George Klein wrote of "self" and "meaning schema." The former indicated "a grade of organization," "the experiencing person," while the latter referred to "the unit of cognitive organizations as they exist internally as motives in a person." This concept of the "self" schema is congruent with Bartlett's "unitary mass" and is one of the cornerstones of the Slap and Saykin model. From this "self schema," nonintegrated schemata can be *sequestered*. This occurs in pathologic conditions such as neurosis, as for example when "the repressed or dynamic unconscious is sequestered." This is consistent with Anna Freud's observation that only in situations of intrapsychic conflict is "a cleavage [between] id, ego and superego . . . discernible.' " "Sequestered schema" is used interchangeably with pathologic schema. Further, as Klein outlined, schemas can have their own "drive" character such as "the cognitive record of the experience of sensuality [in an object relationship]." This constitutes a "meaning schema" which can be "persistently activated under certain conditions [thus] giving the schema its drive

characteristics." According to Klein, these would be examples of the "basic biologic tendencies which the concept of id was supposed to accomplish."

Schema development and activity were further described in accordance with Piaget's concepts of assimilation and accommodation: "assimilation is the conservative process involving perception of, and action on, the environment based on prior structuring, while accommodation is the radical process of reorganizing one's structures in response to new experiences and modes of interaction with the environment." Therefore, schemas are modifiable in response to new experiences and at the same time help shape the character of current experiences. This "applies to memory, learning, intelligence, and the growth of personal relationships. The concepts are not metapsychological and require no assumptions concerning hypothetical energies; they are descriptive." Consequently, in the pathological situation, new stimuli might be assimilated but not modify, i.e., accommodate, the existing self schema. In that case, a state of disequilibrium, in which new data might be sequestered into an organized schema, would exist.

The authors cited the discrepancies and inconsistencies in the tripartite structural model. The terms id, ego, and superego are used "under a pseudoconsensus of meaning." In support of this contention the authors cited the confusion over the concept of the id among participants in a panel on the id reported in 1963. They also cited Gill's finding that primary process functioning can be found in the ego and that the id is not totally without structure. They therefore concluded that there is no neat separation between id and ego: "motivational strivings and the defenses against them cannot be separated into two systems and define the essential characteristics of the systems." The ego and the id are actually similar and would better be defined as schemas. The ego would be replaced "with the concept of the unitary or self schema, a concept similar to the topographic model, and . . . what Freud called the main mass of ideas; this would encompass the individual's perception of himself, his knowledge, his talents, his goals, and his moral code, insofar as these elements are integrated and available to the individual. The concept of the dynamic or repressed unconscious would be replaced by that of an unaccommodated schema sequestered from the self schema."

The authors then attempted to integrate clinical phenomena into their model. They began with the defenses, namely, repression. Repression is considered to be a paramount defense which at some point in the child's psychological development overrides all other "secondary" defenses, e.g., reaction formation, identification, regression. Repression separates the unitary self schema from those "organized conflictual" complexes which arise at various levels of development. These complexes or "sequestered schemas" contain temptations, affects, "secondary" defenses, etc., which as they originate in the course of development are, if nothing happens to alleviate their establishment, "internalized" and thus exist as "walled off," pathologic entities. According to the proposed model, the failure of this walling off would be understood as the equivalent of "the return of the repressed." Thus, the sequestered, pathologic schemas would have "freer access to consciousness and play a greater role in the mentation and behavior of the patient." In contradistinction to Freud's topographic model, in which "secondary" defenses are seen as "last ditch efforts of a beleaguered ego against emerg-

ing instinctual forces," i.e., when repression begins to fail, the authors maintain that the "secondary" defenses are part of their schemas and become more manifest as these schemas influence behavior. These defenses have their origins in "pre-repression" development. Thus, "secondary" defenses influence behavior because they are part of an emerging schema and not just "last ditch efforts." In the borderline patient, the breakdown of repression is more complete, and more primitive defenses are involved. In psychosis, reality is assimilated by the pathologic schemas and distorted or replaced. This becomes more apparent as repression breaks down.

Concerning other clinical phenomena, such as the infantile neurosis, transference, working through, and termination, the sequestered schemas that evolve during childhood continue to effect perceptions and experiences by assimilating them and simultaneously influence manifest behavior. Among the current perceptions that are distorted as they are assimilated by the pathologic schemas are those of the analyst, i.e., transference. Through interpretive work, the barriers to sequestered schemas are diminished, and the self schema is able to expand as it accommodates to the current reality which no longer is pathologically assimilated by the sequestered schemas—this would be working through. "The goal of psychoanalysis may be stated as the gradual integration of such schemas into the self schema." Criteria for termination can then be seen not just as "making the seemingly boundless unconscious conscious," but as the extent to which the self schema can expand and influence behavior, as the definable sequestered schemas are reconstructed.

DISCUSSION: Dr. Martin H. Blum agreed with the authors that there is significant uncertainty regarding the language we use to discuss the fundamental categories in which we group the materials that emerge during analysis. Elaborating on the authors' thesis, Dr. Blum stated that optimal personality function depends on the integration of self schema with the previously sequestered subschemas, and can be seen as "the integration and harmonization of the mild multiple personality disorders from which every human being suffers." This idea is not as novel as it sounds but was rooted in analytic theory from the very beginning. Dr. Blum traced some of the early roots by going back to Breuer's notion that "affective ideas" can stand in conflict with the "integrated and synthesized conscious personality." The "affective idea" arose during a "hypnoid state" and was part of a "defective sub-personality" which generated such ideas. The analogues of these "affective ideas" in later Freudian theory are the transference thoughts that Freud postulated around 1900 as he was developing the topographic theory. Dr. Blum outlined Freud's development of the structural hypothesis from the topographic, stressing that these transference thoughts were a "system" intermediate between the systems *Ucs.* and *Pcs.* These transference thoughts were associatively connected to infantile complexes and to the system *Ucs.* and were affected by these complexes although appearing to be part of the system *Pcs.* In this way, they too amounted to a "sub-personality." The transference thoughts of the topographic model became the defensive ego of the structural model of 1923 and essentially comprised a sub-personality, i.e., a personality dominated by defenses against unconscious infantile impulses. Dr. Blum seemed

to suggest that the advent of ego psychology derailed this trend of analytic theoretical development until recently when the theories about borderline and narcissistic personality disorders began to emerge. These theories, in particular the concepts of "denial" and "splitting," address the phenomena of "conflict and shift . . . among various organized sub-units within the personality" and should be added to general psychoanalytic psychology as opposed to being "diverted into subsidiary literature." These theories continue the development of Freud's ideas about "sub-organizations which alternate in the control of consciousness and are probably of wider generality than might be inferred from their origins as descriptions of "primitive" personalities. Investigation of such processes inevitably flowers into an interest in some version of a theory of multiple personality." Dr. Blum then discussed some of the current literature which addresses the issue of multiple personality, including the work of Irene Fast, Harold Searles, Jeanne Lampl-de Groot, and Mardi Horowitz. In conclusion, he stated that the paper by Dr. Slap and Mr. Saykin is steeped in analytic tradition, but is also on the edge of new developments that will pave our way to the future of analytic theory.

Dr. Joseph T. Coltrera stated that the authors' paper is an "epistemologic, iconoclastic reconsideration which proposes a concept of psychic structuralization outside of the [traditional] structural point of view." He felt that they had offered a polemic which seems consonant with the "so-called 'alternate schools'," e.g., Kohut; the British Object Relations School; and Roy Schafer, George Klein, and Merton Gill. "As with Schafer, the authors wish to redefine psychoanalytic theory around the preeminence of the person as agent and maker of meaning" by eschewing metapsychology, particularly drive theory and its "complement of a structural notion of drive restraint." According to Dr. Coltrera, this is essentially a structuralist point of view, in keeping with Lévi-Strauss and Piaget. What the authors failed to realize is that by the time of the 1914 *Papers on Metapsychology,* Freud himself drew "essentially structuralist conclusions" and advanced psychoanalysis into modern psychology by following Kant in granting cognition primacy. That is, through the self and object ideational representations of the drives, the mind can impose meaning on "itself and object experiences." As drive wishes are "known through meaning," they become capable of being interpreted. The problem with the authors and with the critics of metapsychology is that they insist on treating psychoanalytic interpretation as the topographic-economic explanations of the 1895 *Project.* In relying on Piaget to explain the concept of "schema," the authors had neglected to consider that Piaget's theory of knowledge is purely a cognitive, motor, operational theory which fails when transposed to object relations theory because it does not concern itself with "drive derivatives and the affective aspects of behavior." When using this cognitive model to explain, for example, the manner in which "schema" achieves the process of internalization through assimilation and accommodation, the authors rejected the structural point of view. Then, seeming to contradict themselves, they neglected to consider that Piaget himself noted an intervening "structural" variable when he noted in the eight- to twelve-month-old child the "capacity for delay between the initial perception of the goal and the final action." The authors themselves indicated synthetic function when, in quoting Bartlett, they spoke of responses which are "serially organized" as a

"unitary mass." They used the concept of repression while "rejecting the energic model of repression as structuralized countercathexis put forward in the 1915 papers on repression. How, then, do they conceive of such 'repression'; in a conflictual or a nonconflictual, cognitive context? . . . The attack on the structural point of view here is really an attack on drive theory and the notion of unconscious overdetermination." Dr. Coltrera criticized the authors for taking the concept of id, ego, and superego too literally. Sufficient behavioral references to the id do exist, and "the ongoing work of the analysis literally turns on the confrontation and clarification of the drive-organized nature of a critical behavior or thought." The behavioral referents of the id are: preemptoriness, cyclical rising to and falling away from consummation, selective discharge to the object of prime intention, and displaceability to substitute objects in its absence. Since displaceability makes transference possible, transference is a drive-derived term, a notion which the authors ignore. Their work is reminiscent of Fairbairn and, ultimately, Kohut, especially in Fairbairn's contention that "development has to be conceived in terms of dynamic structures based on experience with objects instead of unstructured psychic energies." Furthering his argument in favor of drive theory and the structural point of view, Dr. Coltrera cited the clinical phenomena of working through and mourning as evidence for a continuing concern for "cathectic considerations" in psychoanalytic theory and practice. His main unease with the authors' model, as well as with the ideas advanced by the metapsychological critics, is that they are not as useful in broadening our understanding of clinical states. Dr. Coltrera took strong exception to the "naïve and reductionistic model of termination ascribed by the authors to classical psychoanalysis." Finally, he wanted to know how the model of the "schema" proposed to deal with the resolution of the transference neurosis, "on whose resolution we critically define ourselves apart from all other psychotherapies."

MICHAEL H. SINGER

The Fall Meeting of THE AMERICAN PSYCHOANALYTIC ASSOCIATION will be held December 14-18, 1983, at the Waldorf-Astoria Hotel, New York City.

The 41st Annual Meeting of the AMERICAN PSYCHOSOMATIC SOCIETY will be held March 8-11, 1984, at the Hyatt on Hilton Head Island, Palmetto Dunes, South Carolina.

The Psychoanalytic Study of Society, which began in 1947 as *Psychoanalysis and the Social Sciences* under the editorship of its founder, Géza Róheim, wishes to announce a new editorial policy. Beginning with Volume 11, the editors will be interested primarily in psychoanalytically oriented papers based on studies

originating within the social sciences. Not only are anthropology, folklore, history, and sociology included, but also mythology, religion, and papers in other areas of knowledge—literature, for example—if they are both psychoanalytic and are seen and structured from a social science perspective. Please send four copies of papers for consideration either to L. Bryce Boyer, M.D., 3021 Telegraph Ave., Berkeley, Calif. 94705; or to Simon A. Grolnick, M.D., Department of Psychiatry, North Shore University Hospital, 300 Community Drive, Manhasset, N.Y. 11030.