

OBSERVATIONS ON
THE CLAIM OF
IATROGENESIS IN THE
PROMULCATION OF
MPD:
A DISCUSSION

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ABSTRACT

The term "iatrogenesis" has both intensional and extensional (i.e., connotative and denotative) meanings which are frequently confused. While the four previous papers of the David Caul Memorial Symposium on iatrogenic issues in multiple personality disorder explore the extensional sense of the term, the discussant of this symposium focuses on the "iatrogenic debate" over MPD in its intensional form, augmenting the scope of the discussion considerably. His comments are based on extensive conversations with David Caul about the subject during the year preceding Dr. Caul's untimely death.

"It ain't over until the fat lady sings," David Caul used to say. I feel like the fat lady now, the last act in a long show.

The presenters in the David Caul Memorial Symposium on Iatrogenic Issues in MPD — Drs Philip Coons (1989), Catherine Fine (1989), Richard Kluff (1989), and Moshe Torem (1989) — have covered the truly iatrogenic issues in multiple personality disorder (MPD) so thoroughly, from so many points of view, and from so much richness of clinical experience and case presentation, that it is impossible to review them without redundancy.

There is agreement among all presenters that: 1) iatrogenic artifacts related to MPD can be found in certain diagnostic and treatment procedures; 2) those attentive to therapist-induced artifacts can learn to avoid them; and 3) artifacts generated by MPD patients' subtle defensive strategies cannot be altogether avoided, but they can be recognized and ameliorated.

As a discussant, the critical analyst of a symposium of ideas, the harmony of the foregoing papers would leave me with nothing to say; except that in this role I have the concurrent responsibility of amplifying and supplementing what has already been stated about the subject at hand.

In this regard I am fortunate, rather than speechless, for the subject of iatrogenesis in MPD was one of the topics David Caul and I spoke of at length during the year of his election to the presidency of the International Society for the Study of Multiple Personality and Dissociation (ISSMP&D), a position I held at the time.

Dr. Caul's concerns about iatrogenic issues in MPD lay mainly in two areas. On the one hand, he was concerned that multiplicity was being overdiagnosed by therapists who were neophytes to the field either to attain narcissistic gratification at "having a multiple of their own" or through despairingly giving a difficult and confusing patient the label of an illness known to be treatable. Dr. Philip Coons has touched nicely on the area of therapist variables in misdiagnosis.

On the other hand, David was quite concerned about the whole irrational debate about the so-called iatrogenic origins of MPD. David knew full well that clinical MPD could not be created even if one tried, but he wanted to get the whole issue out in the open, "within the rank and file of the Society," to use his phrase.

Caul was very concerned that the major critics of MPD were assuming what were merely speculations and conjectures as scientific matters of fact, then using these assumptions to "beat up on" capable, responsible therapists and their patients.

David's ultimate concern was not with the scholarly debate itself, not even with the welfare of therapists, but with the adverse impact the iatrogenesis "accusations" were having on already-diagnosed MPD patients: that of demoralizing them, devaluing them as suffering from an unreal illness, frustrating them in their efforts at receiving treatment.

Caul had spent the bulk of his career in the state and community mental health system of Ohio, and served the system with the dedication and conviction of a career military officer. The bureaucracy itself David often disliked, at times immensely. But what the public mental health system stood for — affordable, quality mental health care for everyone — he believed in and was an ardent spokesman. For all the restrictions he felt, he accomplished some remarkable things during his career.

Thus, Caul's concerns with the iatrogenesis debate were but one concern in his larger body of concerns about MPD patients' welfare. David assumed the presidency of the ISSMP&D with a mission, an agenda, and a vision. Had he lived out his presidency, and his past-presidency responsibilities, we would have seen the full details of his vision. He held his detailed plans close to his heart, I believe, but I know the scale of his mission. David's final professional dream was

that all MPD patients everywhere could receive at least informed, and ultimately competent treatment. The iatrogenesis issue was a stumbling block he felt he had to remove.

This is my first experience in what is partially a posthumous collaboration with a revered colleague; I hope I represent our integrated views fairly.

RETHINKING THE ISSUE

One of the curses of classical education is that one learns to think logically; the curse is doubled when one obtains an advanced degree in philosophy, for one learns to think logically within any system of logic. By the time I was 24 and had obtained my master's degree in philosophy and was appointed a university instructor on the subject, I realized I was completely ill-prepared for the world of people, who seemed rarely to think logically at all. Having discovered my mistake, I sought doctoral training in clinical and social psychology where quite irrational matters are researched in most systematic ways, even if not always logically.

If psychological training didn't harm me; my philosophical training positively ruined me.

I think to myself, Richard Kluft deftly shored the sheep of iatrogenesis in 1982, followed by confirmation by Bennett Braun in 1984. Why now are we having again at the twice-shorn beast, this time in a full symposium?

My thoughts turn to my systematic studies of Ludwig Wittgenstein, an Austrian-British twentieth-century analytical philosopher who taught at Cambridge University until his early death at the age of 52.

I paraphrase him:

"Problems in [knowledge] start with the formulation of the problem . . . when problems persist they are ill-conceived problems from the start . . . the problems of philosophy are problems in the conception of the problem." (Wittgenstein, 1950)

The "buzzword" in the Caul symposium is "iatrogenesis," meaning literally "of physician origin." In Webster's Ninth New Collegiate Dictionary iatrogenesis means "induced inadvertently by a physician or his treatment." "Inadvertent" means both: 1) inattentive and 2) unintentional. Both issues are covered thoroughly in the papers above.

But the papers above, particularly Kluft's (1989) contribution, clearly state that MPD proper cannot be created either by misattention, inattention, unintention, or even by intention on the part of psychotherapists or researchers. We seem to be back to square one.

There is, however, a solution to this sticky problem.

IATROGENIC EPISTEMOLOGY

Aristotle made the distinction between remote and proximate causes, a distinction still made in both science and the law. Following Plato, he also distinguished between matters ontological and those epistemological.

In my opinion, we must give the devil his due in this argument. We must freely admit that physicians and psychologists and other trained mental health professionals are the proximate cause of what Myron Boor (1982) has called

the "multiple personality epidemic," though Boor had no idea of the magnitude of the problem when he coined the phrase.

But the iatrogenic effects in multiple personality in the 1980s are not ontological in nature as unnamed critics would have us believe — we have not been creating multiple personality willy-nilly ontologically — we have instead been creating multiple personality epistemologically and, I might add, systematically.

As I pointed out recently at the First Eastern Regional Conference on Multiple Personality Disorder (1989), in my "History of MPD" address, nothing exists in the realm of knowledge until it is first described. In this important sense, Eberhardt Gmelin (1791) should be credited with the invention of MPD, by being the first to have both described and named the prototypical disorder (*ungetauschte Personlichkeit* or "exchanged personality"). Following science and medicine, we might just as well today be calling MPD "Gmelin's syndrome," for which we might be better off; for as both Kluft (1985; 1988) and Hicks (1985) have pointed out, the contemporary term "multiple personality" is disturbingly paradoxical, a confusing oxymoron.

In 1980 multiple personality was reinvented epistemologically in a series of five works which essentially described and defined every aspect of "Gmelin's syndrome," from the history of the illness to its subjective phenomenology to its etiology to its objective phenomenology, unique diagnostic symptoms and signs, and its treatment (Allison, 1980; American Psychiatric Association, 1980; Bliss, 1980; Greaves, 1980; Rosenbaum, 1980).

These works, in turn, laid the groundwork for a plethora of published articles, scientific meetings, professional training seminars, and hospital treatment programs, culminating in the founding of the ISSMP&D and the launching of *DISSOCIATION*, the official journal of the Society.

Such a "grass roots" movement, as Richard Lowenstein (1989) calls it, is unusual in the history of psychiatry. Contemporary interest in multiple personality (post-1943) has not arisen from the writings of heralded professors or from the collaborative esoteric researches of major universities. Instead, it has arisen from the amalgamated observations and research of many independent, highly-trained medical and psychological scientists, ranging from New Zealand and Australia to Holland and Canada and throughout the United States. If the lack of a university sponsor for this research is cause for suspicion among those professors who pooch-pooch the movement, it is precisely the exquisite agreement between independent observers which gives the movement its robustness.

When faced with the specter of hundreds of clinicians diagnosing thousands of multiple personality cases in the 1980s — when in the 1970s there were but a few dozen cases, and before that, many years separated individual case reports — skeptics who have not followed the development of the field closely have naturally been suspicious. But instead of following up on their suspicions, many have resorted to authoritarian, rhetorical denial (e.g., Thigpen & Cleckley, 1984). While no one has yet used the term "organized iatrogenesis," I have overheard grumbling private conver-

sations in my many travels to professional meetings which translate generically into "they are all dupes," referring to clinical researchers in the field. What, one might ask, does that make them who have written off the research without reading it? Since that is a rude question I will not pursue it.

A DISORDER WHOSE TIME HAD COME

It is certainly a legitimate question as to why a disorder believed to be tottering on the brink of extinction should suddenly be described prolifically. Why should there be an explosion of fireworks seemingly out of nowhere in 1980, followed by a dazzling array of publications in 1984 and beyond?

In the forward to Trula LaCalle's *Voices* (1987), I undertook to understand and explain the conditions under which the rebirth of interest in multiple personality occurred.

During the biological revolution in psychiatry during the 1960s and 1970s, it became apparent that drugs capable of blocking dopamine uptake sites in the brain were far superior to any known form of psychotherapy in producing rapid, remarkable improvements in bizarre and chronic symptoms in many cases of schizophrenia; and that certain severe, intractable depressive symptoms were vastly improved by drugs that either stimulated increased production of norepinephrine, another neurotransmitter, or impeded its metabolism.

One could add to this list the neuroendocrine studies of manic-depressive (bipolar) states and cerebrostructural studies of acute anxiety states, until the most optimistic biological psychiatrists could see an end to anything but "supportive psychotherapy" by the mid-1980s, needed only to cheer patients on and help them adjust to their chronic residual psychological disorders that were secondary to their primary brain dysfunctions.

A psychiatrist colleague of mine sniffed in the air one day and said, "I think it's an affront to human dignity to probe into persons' private lives. You merely have to treat the symptoms of what bothers them."

While the whole dream of the absolute biological predominance of psychiatry was at the same time amusing and alarming, and never posed a serious threat to the field of psychotherapy, it did create a shift in the perpetual nature about the ultimate combining factors in human behavior which drove many psychodynamicists back to the drawing boards.

NO PILL OF ANY COLOR

An old Chinese proverb should somewhere say: "Child who fall in well for three days, not be cured by herbs alone."

Every human being on earth can understand the last common-sense statement. We all know that people can have "shattering experiences," "be shocked to pieces," "have a numbing experience," "a totally destructive experience," or an "experience that eventually drove them to suicide." The common wisdom of mankind's knowledge, as expressed in ordinary words we use, is that "shattering events, whatever they are, must be put behind" if one is to get on with life. We

also acknowledge that "you've got to talk about it in order to put it behind."

Psychotherapy was never in trouble from the new biological discoveries because the ancient roots of applied psychology lay in the knowledge of the power of specific forms of verbal communication to heal, to soothe, to console, to facilitate the grief process, to aid in understanding. To eradicate psychotherapy from the world of the psychiatric healing arts would be to decimate human language—the principle form of substantive communication among people—and all the healing secrets psychotherapists have gleaned from it.

The phrase, "Sleep, my child, and peace attend thee" has no counterpart in benzodiazepine therapy.

When I was a post-doctoral student and clinical instructor in a major department of psychiatry, the cynical song of the residents went: "Stun 'em or drug 'em . . . but make them forget." Nobody believed that, of course, but that's one of the things that was taught as orthodox psychiatry. If you couldn't remember what you were worried about, you couldn't be worried, hence you'd be less talkative about matters which made you anxious and would bother other people less.

In this context, induced amnesia was believed to have a positive benefit, never mind the fact that gross disturbances of memory has forever been considered a cardinal sign of major mental dysfunction. This situation creates a bit of a paradox when discussing iatrogenesis: intentionally-induced amnesia is orthodox, inadvertently introduced amnesia is not.

THE WOMAN WHO WAS BURIED ALIVE

I received a patient on the inpatient rotation during my post-doctoral training who alleged, during our initial clinical interview, that she had been kidnapped from her college campus, raped and buried alive. Because of this harrowing experience, she believed, she had thereafter suffered chronic nervousness, nightmares, flashbacks, insomnia, periodic disabling depressions, hospitalizations on the anniversary date of the event for the past seven years, ranging from two to four weeks in length, could not tolerate intimacy with men thereafter, had lost her job because of her "jumpiness," and was now living on long-term disability insurance, in a vegetative life-style she reportedly loathed.

On closer probing her earlier remarks became ever more detailed, if more implausible. She had been dug up, moved, raped again, buried a second time; then dug up, moved to an abandoned building, raped repeatedly by her abductors for days, then finally escaped into civilization while they were asleep, pretending earlier that she was sound asleep.

She was 27 years old, intelligent, never married, attractive, well groomed but not expensively dressed, worn down, and completely non-psychotic through a two-hour interview. The only thing I knew about her was from the face-sheet of her hospital chart. Past diagnoses had included paranoid schizophrenia, psychotic depression, and hysterical neurosis—representing three of the most diverse clinical

descriptions of a human being I could imagine. The provisional diagnosis of the unit director who had agreed to admit her was paranoid schizophrenia. I tried to find the symptoms; I couldn't. I couldn't find psychosis of any kind; I couldn't find hysteria of any kind.

The patient, sensing my perplexity, reached into her purse and withdrew a sheaf of newspapers clippings. "Perhaps these will help," she said.

I took the stack of clippings to my office and read them, late into the night.

They were the newspaper transcripts of her abductors' trial, pictures of her abductors, scenes of the burial sites, and pictures of the abandoned building. The police had been able to reconstruct every detail of her story with physical evidence. The abductors had been given life sentences.

The next morning when I presented her case to the senior clinical staff and my fellow students I was praised for discovering that this poor woman likely did not suffer from any of the illnesses previously diagnosed, but that she suffered from a "traumatic disorder" instead. She was kept under observation for several days, administered a tricyclic antidepressant (imipramine), and discharged. I felt I had done something quite helpful.

But irony is the devil of man's reason. For in keeping the "buried alive" woman from the misapplication of phenothiazines and electroconvulsive therapy and ineffective hospitalization, I unwittingly bought into the completely sophistic notion that people suffering from "actual trauma" were inherently resilient.

I now know the woman suffered from post-traumatic stress disorder and, appreciate that given the "state of the art" at that time, would only receive band-aid treatment for years to come.

This case is cited by way of illustrating how little was known about traumatic disorders in 1970 in a major center of academic psychiatry, and how little I had learned about these conditions in my years of graduate studies. Except for the "war neuroses" traumatically-induced disorders were believed to be of little consequence and were accorded little attention. Today I find myself on the phone with case reviewers, arguing for the need for extended hospitalizations for certain severely traumatized patients, confronting incredulity and confusion on the other end of the line. Except for the historical accident of my having come to specialize in the dissociative disorders subclass of traumatic disorders, those clinicians I am addressing on the other end of the phone were schooled and trained exactly as was I: that traumatic disorders are the least serious of all mental disorders.

PROFESSIONALS' RESISTANCE TO BELIEF IN MPD

Let me pose what epistemologists like to call a "knotty problem." Anyone with the slightest grasp of biology and physiology can readily understand that if the neurotransmission mechanisms of the brain are faulty, an individual is in trouble and is going to display symptoms either behaviorally and/or psychologically, depending on exactly what is wrong.

Somewhat harder to grasp is that severe, disabling

neuroses can occur psychodynamically from interactive familial processes so subtle that it requires intervention on the part of a highly skilled professional either to identify the processes or to assist in resolving their effects. For all that, psychotherapy and psychoanalysis have become esteemed professions.

Imagine now the case of the individual with intact brain functions who was subjected not only to traumatic psychological interactions as a child, but who was also subjected to severe physical and sexual abuse, neglect, abject humiliation, and a host of other human atrocities toward children. Would there not be an additive effect to instance two above? Could we not conceive that such overtly outrageous treatment might well produce an individual far more injured and dysfunctional than a neurotic?

Logically this must be so, for the effects of trauma heaped on trauma cannot have a subtractive, hence beneficial effect, otherwise we should then have to consider the potential healing effect of trauma.

Laypersons have not the slightest difficulty grasping this notion or any part of it. Why then should psychiatric profession balk at it?

It is not the logic of the above which is in dispute; what is disputed is the proposition that severe child abuse exists, especially when joined with its corollary proposition that it is not so uncommon.

Again I hear harumphs in the hallways: "these child abuse claims in psychotherapy; it just never happens that way."

* * *

A patient tells me she smothered her new-born child two years before, convinced the death would be attributed to sudden infant death syndrome (SIDS). She was correct.

A few weeks later I invite to a party of mine the president of a medical corporation, whose company has just launched a SIDS warning device, and I ask him how his researchers could tell the difference between a mother smothering her child and SIDS.

He lectures me on how SIDS is never homicidal; it is a natural death of non-breathing, due to some neurological defect in the hindbrain. I tell him I am aware of that, then press him on how one can tell the difference between true SIDS and homicidal SIDS. He misses the point again, and I soon realize that he is not being deliberately evasive or obtuse: he simply cannot conceive of a mother murdering her infant child. He slowly disappears somewhere between the artichoke hearts and the iced shrimp, and I never see him again.

* * *

As I recalled the above episode of some years ago, I serendipitously read the following story in the *Atlanta Journal* (1989, July 4, B-3). I précis the account, including quotes where directly quoted.

A mother of four children experienced her children dying,

one by one. The first child "simply didn't wake up on the morning of September 25, 1977. The Fulton County death certificate put it down to sudden infant death syndrome (SIDS). Three years later, Tibatha Janeel Bowen, 3 months old, died in her sleep, and that death was also listed as SIDS, according to medical records. . . . On February 15, 1981, Earl Wayne Bowen, two-and-half, became the third of Mrs. J.'s children to die. His death was blamed on 'seizure disorder of unknown etiology.'" The mother has been arrested and charged with provable, specific misconduct in one of the deaths, possibly two.

Before I could integrate the above notes into this summary paper, Mrs. J. confessed on July 5, 1989 to two of the alleged murders, stating she had smothered the infants by rolling over on them while they slept in her bed (*Atlanta Journal*, July 6, A-1).

This case is instructive for several reasons: 1) 12 years elapsed between the death of the first child and the discovery and confession of the crimes; 2) police and medical experts at the time and place of each child's death did not discover the deaths as crimes and closed all cases; 3) an investigative reporter discovered an apparent pattern of crimes and reported it in the public news; 4) the police and medical examiners reinvestigated the alleged crimes, based on the news report; and 5) obtained a confession.

In the above series of events, the investigative reporter is the proximate cause of the crimes in the epistemological sense, for without his descriptions of curious events there would have been no systematic investigation of the crimes. In the ontological sense of causality, however, the reporter is entirely without crime; the proximate cause is the murderer.

Would that matters worked so cleanly, but the psychological mind grows muddled during peaks of emotion. Soon friends and sympathizers of the woman will rile at the reporter for discovering this woman's actions and he may be filled with guilt for what he has discovered about the woman; and the woman may justify her actions in a manner which will point the source of her difficulties with the law in the direction of the reporter. Like the historical stories of the bearer of bad news being slain in the heat of the moment, it would be unlikely for the reporter to escape blame for his discovery because of the trouble he has caused.

CHILDHOOD HORRORS

Jean Goodwin (1985) was the first to develop the hypothesis that disbelief in MPD among mental health professionals arises as a countertransference phenomenon: that practitioners encountering such patients simply cannot tolerate the patients' accounts of their childhood horror and respond to such accounts with their own primitive and unrecognized denial defenses. Paul Dell (1989) has been making a study of the degrees to which non-believers in MPD take personal and administrative actions against their colleagues who diagnose and treat MPD, indicative of the deep passions of antipathy which somehow get stirred up in these cases, antipathy which cannot be explained in terms of an

academic disagreement.

LEARNING NEW TRICKS

One of the issues that Dr. Caul and I talked about regarding resistance issues was his concern that colleagues were disconcerted about having to learn anything new. To treat an MPD patient is to learn much about the theory, phenomena, and practice of hypnosis; to learn about dissociation; to have to digest a new literature; to learn a new terminology; to have to attend seminars and seek supervision; to have to modify some old ideas. "It's hard to teach an old. . . ." You know the rest. David saw this resistance to learning as both the basis of the proclivity of practitioners to refer MPD patients on once having diagnosed them, rather than learning how to work with them, as well as the deliberate overlooking of the MPD diagnosis, since to make the diagnosis could potentially cause problems for the therapist.

THE IATROGENESIS OF NEGLECT

Dr. Caul's last concern moves me to wonder if there is not one last sense in which we should consider physicians' inadvertent worsenings of illnesses. The extreme length of time reported between initial involvement in psychiatric care and diagnosis of MPD (over six years) and the proliferation of prior diagnoses (between three and four) (Putnam, Guroff, Silberman, Barban, & Post, 1986), have led many MPD patients to despair, to the waste of years of their lives, and to the depletion of lifetime insurance benefits and personal funds for psychiatric care before they first are diagnosed accurately. Caul's hunch that psychotherapists might be deliberately avoiding the diagnosis in order to avoid inconvenience to themselves or their working environment is chilling.

In a section above I spoke of "professionals' resistance" to belief in MPD. David Caul used a similar-sounding but very different term. He called it "professional resistance."

Two brief vignettes suggest that Dr. Caul might have been at least partially correct in his dark hunch.

The first case involves that of a psychotherapist who was under great fire from his peers for insisting that his patient was suffering from MPD while his superiors insisted the patient was schizophrenic. I was brought in at great expense, due mainly to the travel costs involved, as an expert consultant. Among the things I learned was that the patient was flagrantly multiple and had been confronted by the chief of service of the hospital who commanded the patient to "quit dressing funny and calling yourself by a different name."

In the second case I was brought in as a consultant to a hospital which had a policy that it would not admit MPD patients. If a patient were so diagnosed while in the hospital, she would be discharged. As hospital administration has its ways of getting things done, I was called in the afternoon before the patient's scheduled discharge in the morning, though she had been hospitalized and suspected of being multiple for several weeks.

Again, the patient was flagrantly multiple. She got her diagnosis, which no one dared voice above a whisper until

now, and the patient was discharged as planned without the hospital having to incur the potential liability of testing its own policy: that of administratively discharging a patient solely on the basis of a category of diagnosis.

* * *

It is into this area that I see the iatrogenic issues surrounding MPD moving in the next decade: that of the negligent failure to diagnose and treat for MPD. This is my exact surmise of the drum David Caul intended to beat, once this symposium was behind him.

In *Schuster v. Altenberg*, Wisconsin Supreme Court (1988), the court articulated this concept clearly: "Negligent failure to diagnose or properly treat [a] psychiatric condition may constitute cause-in-fact of harm to patient and third parties if it can be established that, with proper diagnosis and treatment, the patient's condition could have been corrected or controlled." ■

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