

EXORCISM AND POSSESSION: THE CLASH OF WORLDVIEWS AND THE HUBRIS OF PSYCHIATRY

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ABSTRACT

Commentary is provided on four psychiatric papers concerned with (1) "trance/possession disorder" (TPD) and (2) "exorcism." It is argued that such papers exemplify a centuries-old clash of worldviews between science and religion. Arguments against the inclusion of the proposed diagnostic entity of TPD are outlined. The criteria for TPD are considered too vague for practical clinical use in differential diagnosis and may incite an epidemic of "possession" in North America if included in DSM-IV. The diagnosis of TPD may also enhance the unconscious institutional racism and ethnocentrism of psychiatry. "Exorcism" outcome studies that purport it is harmful to MPD patients are criticized for their lack of operational definitions, selection bias of subjects, and biased framing of quantitative data. Issues regarding the biased framing of data in scientific publications in general are discussed.

My comments on the four previous papers are concerned with the following issues: (1) the necessity of a diagnosis of "trance/possession disorder"; (2) the ramifications for our culture if it is adopted; (3) the problems of scientifically determining the outcome of exorcism; and (4) my general criticisms and concerns regarding the scientific status of the language used in these and other psychiatric papers and the framing of information in such papers. I had initially planned to adopt a more extensive anthropological critique of these papers, but such an approach has instead been masterfully conducted in the contribution by Begelman (1993) above.

Given the "Satanism scare" hysteria in our culture and at the highest levels of authority in psychiatry, I greatly fear the direction psychiatry may take if some of the ideas in these papers become accepted as "scientific" opinions. Our culture looks to psychiatry and clinical psychology as the definers of reality to a large degree, whether we in the profession like it or not. If we accept the premises of three of these authors (Coons, 1993; Bowman, 1993; and Fraser, 1993) concerning the adoption of "trance/possession disorder" as a new diagnostic category and the purported harmful effects of "exorcisms," I fear that our society will soon have an epidemic of the possessed, and a turf war will commence between

mental health professionals and religious specialists about who should provide the best treatment for the possessed. Perhaps what troubles me the most, however, is that these three papers on possession and exorcism may herald the next wave of hysteria in psychiatry (and therefore in North American culture) that may actually eclipse the Satanic ritual abuse controversy in the wildness of claims and in the numbers of cases.

The return of "possession" as a medical diagnostic category would mark its first "official" recognition since 1838, when the great French *aliéniste* J.E.D. Esquirol proposed "demonomania" as a category of mental alienation (Esquirol, 1838/1845). Most human beings have a gut level intuition — if not an experience — of what being "possessed" is like. A sudden downward, perhaps paralyzing, shift in mood; a blinding (but brief) moment of rage; the all-too-real, almost audible, internal dialogue that babbles incessantly on and on in many of us, and is especially pronounced in times of great moral conflict. All of this can be easily interpreted, depending on one's belief system, as quite convincing signs that one is "possessed" by some autonomous agency within one's own psyche. Human beings "recovering" memories of childhood abuse at the hands of "Satanic" family members are numerically a drop in the bucket compared to potential cases of trance/possession disorder that will appear if it is legitimized as a DSM-IV mental disorder. I can imagine a future in which the concept of "possession disorder clinics" with religious specialists on staff from all denominations as part of the mental health treatment team could be the next creation of private psychiatric hospital marketing departments in the 1990s if trance/possession disorder is given official sanction by the American Psychiatric Association.

This brings us to the question: Is there such a mental disorder as trance/possession disorder? Coons argues in his paper that there may be need for such a category. Unfortunately, after reviewing the cross-cultural references in his paper, the diagnostic criteria for the proposed disorder, and the argument for creating such a new category of mental disorder, in my opinion there is no need for it. I find nothing in this paper that convinces me this is a necessary diagnostic category. My conclusion is based on several points, some of which go beyond Coons's paper, especially those hypothetical situations that are imagined when one asks "What if...?"

It is true that reports of "spirit possession" as an interpretation of psychopathology are prevalent in most Third World cultures. Indeed, within the folk psychology of mil-

lions of persons in our own culture, a belief in the possibility of "possession" by "spirits" is probably not uncommon. However, how prevalent a distinct "possession disorder" is in North America is perhaps unanswerable.

Is the role of *DSM* to diagnose the whole world or just North American/Western European culture? *DSM* is used widely on this planet, and not exactly with the rigor that is recommended in the manual and which was the procedure followed by those researchers conducting validation studies of the various diagnostic categories. As in this country, cases of "possession" around the world are often given that label within the context of a specific metaphysical worldview that is almost invariably spiritual or religious in some way. A *DSM* category of trance/possession disorder may be nothing more than an attempt to stamp the imprint of "Science" on the territory of its ancient adversary, "Religion," yet another skirmish in the centuries-old war between competing *Weltanschauungen*. Or, perhaps what is at issue is the intimate connection between how a culture often reflects its views of Divine Nature in its views of human nature. Since *DSM* is increasingly used in many countries with traditional cultural groups that have a strong heritage of polytheism and belief in possession ("good" and "bad"), is the inclusion of the proposed diagnostic category an ethnocentric bias, a latent monotheistic prejudice that polytheism (and its attendant advocacy of the normalcy of polypsychism in humans) causes or indeed is "illness"? Would such religions or racial bias implicitly enter the use of the proposed category by clinicians in our own culture, who are overwhelmingly white and Judeo-Christian monotheists, when confronted with patients who are not white and who may be from polytheistic "Third World" cultures or heritage? My suspicion is that, at least initially, the vast majority of persons diagnosed with "trance/possession disorder" will not be white and that this proposed diagnosis will be criticized — and rightly so — as a weapon of unconscious institutional racism.

Would "trance/possession disorder" really reflect a presumed growing number of cases of "possession" in North America that would justify the creation of this new diagnostic category? Probably because it *hasn't* been in *DSM* so far that such disorders haven't been diagnosed in North America, but such interpretations of psychopathology seem never to have been as prevalent here as, say, in Zambia. Culture does matter when interpreting human behavior, and mainly white, male, American, and Canadian psychiatrists of largely Christian and Jewish backgrounds are listed as participating in the committees who have traditionally set the *DSM* diagnostic criteria for the dissociative disorders. So we must ask again: Is such a new category *really* necessary? Given our culture and our scientific and monotheistic cultural biases, do we really understand what we are proposing?

In returning specifically to Coons's paper, the strongest argument that I could find that Coons makes for a "trance/possession disorder" that is distinct from multiple personality disorder and other dissociative disorders is that *ICD-10* includes such a category. Although the opinions of those distinguished clinicians who determine the structure of *ICD-10* cannot be ignored, the appeal of including a homolo-

gous category in *DSM* for the sake of conformity is not a compelling reason for doing so.

It may well be argued that MPD already *is* our Judeo-Christian culture's version of "trance/possession disorder" or TPD. I, for one, would probably argue this. The criteria Coons provides in his paper for TPD are not different enough from MPD or any "atypical dissociative disorder" to allow the average clinician who does not read the supplementary psychiatric literature to make a sound differential diagnosis *in practice*. I would argue that the proposed TPD criteria, as presented, are not a good enough "tool" to be of assistance to any clinician. I ask any reader to re-read the proposed TPD criteria, and then the standard MPD and "atypical" criteria, and try to imagine patients who fit each of them. How different would they *really* look? Perhaps my imagination is rather concrete, but it would be extremely difficult, in practice, to tell these patients apart — especially the TPD and "atypical dissociative disorder" patients. I suspect a validation study for the proposed disorder would present numerous problems of interpretation for those researchers responsible for collecting data.

The vagueness of the proposed TPD criteria may very well enhance the conditions under which the average, well-meaning clinician selects this diagnosis instead of others because of unconscious ethnocentric or racial or religious biases. The study of the operation of implicit memory by cognitive psychologists has demonstrated time and again that, especially when presented with ambiguous stimuli, we are unconsciously guided in our decision-making by previously learned material (see Stein & Young, 1992). We tend to fall back on the prototypes provided by our own personal and especially cultural categories of meaning when making decisions, and this would certainly hold true for making diagnostic decisions.

Coons's assumption is that clinicians are capable of differentially diagnosing possession states and therefore he provides clinicians with information that is intended to help them do this in Table 1. When compiling information to make a differential diagnosis between TPD and MPD, and culturally sanctioned religious possession trances, should we use "age of onset" as a factor? Coons does so in his Table 1. One might well imagine that, if they saw this chart, most Pentecostals, Voodoo practitioners, Brazilian spiritualist mediums or channellers would take great offense at the pathologizing "age of onset" language which is usually reserved for physical and mental *disorders* or *diseases*. Discussing culturally sanctioned altered states of consciousness in this manner is, unfortunately, misleading and devaluing. Furthermore, it entirely ignores the role that culture plays in teaching the sanctioned roles that a possessed person plays and instead implies that possession is induced at a particular age by non-environmental factors. "Age of onset" for possession trances is a "learned" response and not bound to a particular age in all cases. Presenting the cross-cultural phenomenon of "possession trances" in this manner unfortunately makes it appear more akin to diseases like multiple sclerosis or schizophrenia, which have fairly discernable age ranges of onset induced by significant genetic/biological factors large-

ly (but not entirely) independent of environment.

To the naive reader, a psychiatric article with a detailed "chart" or "table" makes the conclusions drawn from the organization of information in the chart seem "scientific," when in fact — as in this case — it may be based on conjecture and incomplete scholarship. On anthropological grounds I would seriously question the implicit assumptions of Table 1, and my reading of the anthropological and sociological literature does not, in my opinion, in any way, support the "ages of onset" concept listed for each of these so-called "trance states." In addition, the language used in Table 1 may be insulting to those various groups.

There are other problems with the information in Coons's Table 1. For example, claiming that "mediumship/chanelling" does not involve "ritual" may be argued to be incorrect, depending on one's definition of "ritual." No one seems to be able to agree on a good operational definition of ritual in anthropology or in psychiatry. What exactly is the meaning of "ritual" in the pseudoscientific term "Satanic ritual abuse"? A "ceremonial setting"? And just what are the parameters of an operational definition of a "ceremonial setting"? Also: Arguing that the "nature" of the cross-cultural trance types can be a useful factor in differential diagnosis is flawed if one uses the distinctions drawn in the table. Stating that the trance of the Brazilian spiritist is "spiritual" in nature, whereas that of the medium or channeller is not, but instead "magic/occult" in nature, is meaningless. Indeed, it is probably insulting to all those thousands of Christians in North America who frequent the spiritualist churches of the Spiritual Frontiers Fellowship (among others) and receive "readings" from "mediums" in prayer/church services that have a Christian flavor.

Using the criteria of whether an altered state of consciousness is "voluntary" or "involuntary" is a gray area, not a clear-cut issue. The Freudian concept of "secondary gain" should certainly be remembered here. There are multiple levels of motivating factors in the inductions of ASCs. Indeed, there are multiple opinions about how many *different* types of ASCs there are: an infinite regress of dissociations that seems to be nothing more than the old Scholastics' metaphysical problem of the "one" and the "many." Anthropologists and others (myself included) have sought in vain to find operative definitional criteria for voluntary or involuntary trance and have not been very successful. We should all be more aware of the literature on *implicit memory* and *parallel distributed processing* in cognitive psychology to keep us humble about what is "conscious" or "voluntary" or not in human information processing (McClelland, 1988; McClelland & Rumelhart, 1986).

Let us turn our attention to the purported "exorcism" outcome-type studies of Bowman and Fraser. Both studies are based on a legitimate humanitarian concern that forms the basis of the conclusions of their respective studies: "exorcisms" performed on persons with MPD seem to ultimately increase their suffering. The first problem with these two studies is the selection bias in the subject pool: MPD patients who were successfully treated with "exorcism" would probably never come to the attention of psychiatrists like Bowman

or Fraser who do not perform exorcisms in the first place. They would have been "healed." Why then would such persons have then gone to clinicians like Bowman or Fraser if they had experienced symptomatic relief? Such clinicians are only going to see the damaged and distressed "failures."

Until we can state conclusively with controlled outcome studies that "exorcisms" only "hurt" and never "help" MPD patients, articles like Bowman's and Fraser's seem only to be "sour grapes": i.e., only trained mental health professionals should be treating those suffering persons who complain of "possession" or behave "possessed" — and no one else, especially someone with a religious worldview who believes exorcism is acceptable as a treatment technique.

When worldviews collide, as they do here between psychiatry and religion, the competition is fierce for establishing who the true authority should be. Both Bowman and Fraser take it upon themselves to "caution" (Fraser) or "educate" (Bowman) any "potential exorcists" (Fraser) especially in the "conservative Christian community" (Bowman). Are they really that sure, based on their biased samples and the incomplete scientific evidence concerning "exorcisms" that now exists, that they are in a position to render such advice? One may argue that such firm assertions are evidence of the hubris of psychiatry, perhaps its unconscious "institutional scientism," derived from its insecurity over being unable to satisfactorily answer from a *scientific* point of view ancient problems like "spirit possession."

From the phenomenological point of view (especially of a suffering patient), is there truly a difference between an "ego state" and a "spirit" or "demon"? No PET scan or MRI images exist that could answer that question, even though we have neuroimaging evidence of differences between "alters" in MPD patients. When potential exorcists are cautioned by psychiatric authorities with advice like, "ego states can be frightened or coerced to believe they are evil entities or spirits and will act out their perceived roles" (as Fraser admonishes), it should remind us that we must again be very careful about the use of language in scientific publications and the presentation of facts. What have we *really* added to human knowledge or science when we make statements like this in the MPD literature? Such statements permeate the MPD literature. However, haven't we just translated concepts from one language to another more acceptable one that matches the prevailing worldview of our time and place in history? But isn't the basic phenomenon the same?

The examination of the use of language in scientific discussions is indeed crucial. For example: What, precisely, is the operational definition of an exorcism? We may as well just substitute the equally elastic and therefore equally meaningless word "ritual" here. Operational definitions of "exorcism" are not provided in the relevant articles in this issue.

"Exorcism" aside, "psychotherapy" has been known to harm people too. Shouldn't it matter *how* an "exorcism" is done before determining whether it is "therapeutic" or not? Again, it is unfortunate that papers like Bowman's and Fraser's that make "scientific" claims end up once again sounding like no more than just "sour grapes": the clash this time is

the turf war over language. (Begelman is far more eloquent than I on this issue.) Psychiatry claims its techniques are "therapeutic" because they integrate, unify, etc., harmfully autonomous "alternate personalities" or "ego states." Psychiatry has no place for "spirits": therefore it cannot remove them. Is abreaction during "psychotherapy" just another term for "exorcism"? Perhaps clinicians should examine the solidity of its model and consider that possibility.

There is, in my mind, a serious general issue that arises in these papers and in psychiatric journals as a whole about the *style* of clinical research reports. My concern is with the growing acceptance of a style of presentation in which information is *framed* to appear to be more "scientific" and with larger "effects" than may actually be the case when more closely examined. Ideas are advertised and marketed in scientific journals all the time through such framing, although we all would take offense at anyone's suggestion that it was our intention to deliberately slant our supposedly "objective" results. In many published papers we have information presented in tables, charts, and all-too-frequent references to percentages when discussing small sample sizes (e.g., N=15 for Bowman, N=7 for Fraser) which can bias the casual reader (and most *are*) into getting the impression that a bigger effect is being reported than is actually true. For example, Bowman's abstract reports, "Initial reactions to exorcisms were negative in about 80% of hosts and alters and positive in 14% of hosts and 9% of alters." Only fourteen subjects out of fifteen underwent exorcisms. With such a low sample size, why not just list the exact numbers? This is not just a criticism of Bowman, but an indictment of the way many editors of refereed scientific journals allow results to be reported.

The style of presentation of scientific information *does* bias the reader—even the non-casual ones. Using percentages in reports with small subject pools inflates the effect in the mind of the reader, and when this article is cited in future publications by authors sympathetic to the cited author's perspective, a "snowball" effect can occur and a body of "evidence" will be cited in even more temporally distant publications in the future. Most readers do not go back and check the accuracy of a one-or two-sentence summary of a cited article. They are too busy skimming and retaining the overall methodology and conclusions of a study they are reading. Therefore, the onus of responsibility lies on the author of scientific papers to not overstate one's conclusions and on editors to "see through" framing effects.

For example, from future authors sympathetic to Bowman's point of view we can expect thumbnail summary statements about her research such as "Bowman (1993) reports that initial reactions to exorcisms were negative in about 80% of hosts, etc.," or "Bowman (1993) reports that 93% of persons in her study who underwent exorcisms reported 'painful' or 'bad' feelings or experiences." Then, suddenly, a purported "scientific" literature of "facts" supporting a particular position snowballs into a larger and larger pseudo-truth through fragmentary citations like this in subsequent publications. Given the small sample size, and its highly biased nature, such statements purporting to be the summaries of

scientifically-derived conclusions would be hyperbolic at best.

I have saved Begelman's paper for last for several reasons: (1) it considers multiple theoretical perspectives; and (2) because I seem to agree with almost everything in it. Indeed, with its publication in *DISSOCIATION*, I think it is probably one of the best—if not *the* best—scholarly treatments of "spirit possession" ever published in a psychiatric journal.

Begelman's suggestion that "we may regard possession and multiplicity as contrasting interpretations of the same data base" is a simple, yet almost always forgotten, and *plausible* scientific hypothesis. It makes good common sense, yet emotions run high when people polarize on one ontological position or the other. Perhaps we should remember that this first step of the scientific approach is *observation and description*. We worry about ultimate causal explanations later. An approach like Begelman's keeps us closer to *phenomenology* than to *ontology*, precisely the stage we should be at when trying to figure out the millenia-old problem of "spirit possession." Saying that someone's behavior is causally attributed to the activity of a "spirit," "demon," "god," "ego state," "alter," "complex," "archetype," etc., is the use of language to *seem* causal when in fact it is perhaps just reflective of the belief systems of a particular time or place in human history. When I read articles or books that speak with all the weight of the scientific authority of psychiatry that "alters" or "ego states" are definitely not "spirits," I have to chuckle. But, also, I am inwardly a little embarrassed at my colleagues for their ethnocentrism and ignorance of history. I wonder if the mental health specialists of the 22nd century will look back on the "scientific" MPD literature in the same way we regard as "quaint" the 19th century literature on spiritualism and psychical research. Noam Chomsky once noted that the progress of science since the 17th century can be characterized as perhaps merely the translation of more and more metaphors of the "mental" into the metaphors of the "physical." The basic problems of human consciousness and existence do not seem to change, only the names we give them from one epoch to another. My plea is for more humility in psychiatry. ■

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