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Response

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Response by Alan Dundes

Better Late Than Never: The Case for Psychoanalytic Folkloristics

Utz Jeggle's honest and engaging think-piece on the possible relevance of psychoanalytic theory to the discipline of folkloristics is of interest more for what he fails to say than for what he does say. We have a senior, respected German folklorist who has evidently read some of Freud's writings and decided near the end of his career that maybe, just maybe, some of Freud's insights could possibly illuminate folkloristic data. The "unconscious," Jeggle suggests, might help explain the content of dreams and superstitions. This is surely a case of re-discovering or re-inventing the wheel!

What is sad about this effort is that it is devoid, or should I say totally innocent, of any attempt to consult close to a century's worth of scholarship on ALL the issues discussed and what is even more disgraceful is that the bulk of that scholarship was first published *in German*, Jeggle's native language!

Anyone reading this essay with no knowledge whatsoever of Freud or his followers might easily get the impression that virtually no one hitherto besides Freud himself, and now Jeggle, had ever suggested that there could be an unconscious component in folkloristic forms. There are so many sources available for every one of the topics mentioned by Jeggle that one scarcely knows where to begin.

Let's start with dreams since that is his first example. Freud and a gymnasium teacher D. E. Oppenheim teamed up to write *Dreams in Folklore* circa 1911, a paper in which Oppenheim selected folktales from the pages of *Anthropophyteia* (1904–13), an important folklore journal specializing in obscene folklore, edited by Friedrich Krauss, who was a folklorist described in detail in Raymond L. Burt's *Friedrich Salomo Krauss (1859–1938)*, a monograph in German (1990) not cited by Jeggle. Krauss even asked Freud to write a letter of support for the journal, which Freud did, and in fact, Freud's name appeared on the editorial masthead for volumes 7–9 (1910–12), which is a definite sign of Freud's involvement in folklore research. In the tales selected by Oppenheim there were dreams. The dreams were interpreted in these

tales and the symbolism contained therein turned out to be so-called Freudian symbolism. This was an early statement supporting Freud's oft-repeated claim that the source of a people's unconscious symbolism was folklore. The essay begins: "The symbolism employed in these dreams coincides completely with that accepted by psychoanalysis and . . . a number of these dreams are understood by the common people in the same way as they would be interpreted by psychoanalysis—that is, not as premonitions about a still unrevealed future, but as the fulfillment of wishes . . ." (Freud and Oppenheim 1958). Now it is true that this paper was lost for nearly fifty years, partly because Oppenheim left Freud's Viennese circle after an initial interest in psychoanalysis, defecting to join Adler. It turned up in Oppenheim's daughter's possession in Australia and it was not published until 1958 (in both the original German and in English translation). But the point is that it has been available since that time and it provides better examples of the application of psychoanalytic theory to the unconscious content of German-language dreams than anything contained in Jeggler's discussion of the subject.

Most of Freud's followers did apply psychoanalytic theory to folklore, notably Otto Rank, Karl Abraham, and Ernest Jones, among others, none of whom are named by Jeggler. Perhaps the most glaring omission is Géza Róheim, the first psychoanalytic folklorist, whose early writings were in Hungarian and in German. As for dreams, one might at the very least have noted Róheim's magnum opus, *The Gates of the Dream* (1952). Incidentally, in that work, Róheim has a phallic interpretation of the "legs" in the riddle of the Sphinx discussed by Jeggler. Easy access to several of Róheim's many essays is provided by his book *Fire in the Dragon and Other Psychoanalytic Essays on Folklore* (1992). Anyone seriously interested in any of the topics covered in Jeggler's paper should consult Alexander Grinstein's fourteen volume *Index of Psychoanalytic Writings* (1956–75), at least for the earlier books and articles, including many devoted to superstitions.

With respect to superstition, Jeggler is surely correct in his relating the Freudian principle of the "omnipotence of thought" to that folkloristic genre, but it is a pity that he did not give more concrete illustrations of the principle at work. For example, in American folklore, we have the superstition that "If a person takes an umbrella from home in the morning, it will not rain that day" or "If a person does

not take an umbrella from home in the morning, it will rain that day." Clearly, an individual's thought that his or her action with respect to taking an umbrella from home is going to influence the weather for the entire region is an instance of the principle. Freud himself provides an excellent report of another relevant superstition in a letter of 30 January 1875, written to his childhood friend Eduard Silberstein: "When I was a child, I firmly believed in the envy of the 'so-called gods' and would take care not to speak of fulfillment of a precious wish lest I invoke the very opposite."

One of the best illustrations of the application of psychoanalytic theory to folklore was written by Freud himself in his analysis of Jewish jokes. It was published in 1905, almost a full century ago, but again it is not even referred to by Jeggle. Nor does Jeggle mention the late Gershon Legman's remarkable psychoanalytic coverage of jokes in *Rationale of the Dirty Joke* (1968) and *No Laughing Matter* (1975).

Another area of folklore that has provided convincing examples of the application of psychoanalytic theory to folklore is the folktale, especially the fairy tale. Surprisingly, Jeggle doesn't say anything about Bruno Bettelheim's *The Uses of Enchantment* (1976), a popular book known widely outside the academy, and which successfully brought the application of psychoanalytic theory to the attention of the literate public.

The truth is that although Jeggle does purport to deal with the "unconscious," almost all of his examples are based on conscious, not unconscious reasoning. There is no real insight in his discussion of "lost" objects. Why exactly were they lost? Presumably there are unconscious reasons, but there are no persuasive illustrations. As for the significance of forgetting, Freud's demonstration in *The Psychopathology of Everyday Life*, first published in 1901, had much more convincing examples. I might offer just one instance here. When taking their final examinations at the university, students are invited to submit self-addressed postcards to their instructors so that they might receive their final course grades prior to the official notification sent out by the registrar. On occasion, I have had students turn in such postcards with requests to send them their grades on the midterm, the course project, the final exam, and their overall grade in the course, but they failed to put their name and address on the front of

the postcard. The point is that “forgetting” to do so is a pretty clear reflection of their unconscious wish *not* to receive what might be a failing or at any rate a disappointing grade.

I cannot stress strongly enough that almost every point made by Jeggle has been the subject of extensive published psychoanalytic inquiry, and I don’t really understand why he made no effort to refer to any of it. For instance, he mentions the Kwakiutl potlatch and does refer to Mauss’s 1925 essay, but Mauss’s essay was not the least bit psychoanalytic. Jeggle could, in theory, have consulted my paper “Heads or Tails: A Psychoanalytic Study of Potlatch” which first appeared in the *Journal of Psychological Anthropology* in 1979 and was reprinted in *Parsing Through Customs: Essays by a Freudian Folklorist* (1987) if he really wanted to explore the possible unconscious significance of this seemingly irrational ritual practice.

So while I don’t disagree with much of what Jeggle says, I am disappointed at the low level of scholarship he demonstrates, a level not consistent with the typical thoroughness of standard Germanic academic tradition. And while I am gratified that *finally* a ranking German folklorist has managed to admit that folklorists may have missed an opportunity by ignoring what Freud had to say—for most German folklorists it is as though Freud never even existed—I feel frustrated when I think that readers might glean from his essay that little or nothing has been written along the suggestive lines he proposes.

Let me end this comment by shamelessly mentioning several of my own works, e.g., *From Game to War and Other Psychoanalytic Essays on Folklore* (1997), *Bloody Mary in the Mirror: Essays in Psychoanalytic Folkloristics* (2002), my psychoanalytic monograph *Two Tales of Crow and Sparrow: A Freudian Folkloristic Essay on Caste and Untouchability* (1997), and my psychoanalytic study of Orthodox Jewish culture, *The Shabbat Elevator and Other Sabbath Subterfuges: An Unorthodox Essay on Circumventing Custom and Jewish Character* (2002).

I happen to know that Utz Jeggle is familiar with my book *Life Is Like A Chicken Coop Ladder: A Study of German National Character Through Folklore* (1989), which is also psychoanalytic. I know because he was present when I gave an abridged version of it in Berlin in 1982. I shall never forget his comment to me, made when walking out together after the session. He said, “When I first read your paper, I was very angry. And then I thought to myself, why am I so angry? Maybe there’s something to it after all.” I have always treasured Utz Jeggle’s honesty

on that occasion and his willingness to change his mind or open his mind to new ideas. That admirable quality is also evident in his paper, but I'm afraid it may be too little, too late. I'm not really sure Jeggle's plea represents much of an advance over the final sentence in Freud's letter of support for *Anthropophyteia* written to Krauss on 26 June 1910: "It is therefore safe to hope that the psychological importance of folklore will be more and more clearly recognized, and that the relations between that branch of study and psychoanalysis will soon become more intimate."

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Response by David J. Hufford

Utz Jeggle's entire essay is a bit of a puzzle to me. Jeggle seems simply to take the old Freudian psychoanalytic views as they stand and apply them to folk belief. I share the view he attributes to many, that psychoanalysis is a "discontinued model," *passé* and outmoded, so why respond in detail? Because on the topic of folk belief, *many* "discontinued" intellectual concepts continue to be employed by folklorists, and the difficulties I find in Jeggle's essay are somewhat representative of other applications of theory to belief. Principally these are the lack of interdisciplinary scope (with the result that so much relevant contemporary knowledge is missed) and the strong tendency to begin with theory instead of with a careful examination of the empirical data. I have three main areas of criticism concerning Jeggle's psychoanalytic approach to folk belief. First I will just state them as simply as possible, to give an idea of where my comments are headed. Then I will provide an argument for each of my criticisms.

Jeggle says that he uses his Freudian approach because it can help us to understand certain kinds of behavior, including some aspects of folklore. (I will use the American term "folklore" throughout my comments for simplicity.) That claim must rest on the adequacy and accuracy of Freudian theory as a contribution to an understanding of human thought and actions. Here lies my most fundamental criti-

cism of the approach suggested by Jeggle, and I will offer reasons to doubt both the adequacy and the accuracy of Freudian theory as Jeggle presents it. Second, the area of folklore that Jeggle finds most appropriate for Freudian treatment is “the small madness we call . . . folk belief.” In this connection he finds many analogies between psychotic illness, “superstition,” and religion, stating that psychoanalysis helps us to see “how closely connected religion is to superstitious practices.” This requires us to consider the usefulness of psychosis, as understood from Jeggle’s Freudian perspective, as an analogy to the cultural forms that he finds so closely related. Third, Jeggle uses some extensive examples of dream interpretation to illustrate the applicability of psychoanalytic interpretation both to regular madness and to the madness that he says is folk belief-superstition-religion. This requires that we consider how well supported Jeggle’s view of dreams is, in the context of contemporary knowledge.

I find Jeggle’s case, as presented in this essay, to be without merit on all three issues. In brief, I will suggest that (1) Freudian theory—especially in its classic, original form—today has very little support, particularly in the case of medicine and psychology, the two fields for which Freud created the theory and where he intended it to be primarily applied to generate supporting evidence; (2) the effective use of the madness analogy was attempted, especially in anthropology, for *a long time* in connection with spiritual beliefs and has been largely given up because it fails to fit the actual data—and furthermore, Jeggle’s tendency to equate psychosis with neurosis does not accurately reflect the mature Freudian position; and (3) Jeggle’s comments about dreams are both discordant with modern knowledge about dreams, and they miss specific dream-sleep related research on folk belief—my own included—in a way that harms even his more general points.

A fourth point that I find odd but will not pursue is Jeggle’s preference for classic, old-fashioned Freudian theory. If he is not impressed by the Freudian revisions of the second half of the twentieth century, then why doesn’t Jeggle at least mention Jacques Lacan? Lacan at least represents a reasonably recent effort to revise Freud in a way that is true to psychoanalysis while incorporating contemporary semiotic concerns. I do not find Lacan useful for the study of belief, but some others do. I would expect a contemporary Freudian at least to note Lacan’s efforts.

The Adequacy of Freudian Psychoanalytic Theory

Jeggle acknowledges that today most, certainly most of those in scientific medicine, where psychoanalysis was supposed to find its evidence and its use, consider Freudian psychoanalytic ideas quaint at best. This is presumably the "Freud-bashing" that he says is an American sport. But American medicine gave Freud great scope and utilized psychoanalytic practice extensively for decades. And even since it has ceased to be an important part of mainstream psychiatry, it continues to be widely used in an almost infinite variety of forms by American therapists of many kinds. Consequently, the current "bashing" is at least to be accounted for rather than merely dismissed. Simply calling harsh criticisms "bashing" is not an argument against the criticisms. Jeggle does not argue against them. He does not even note what the criticisms are. He does, however, *illustrate* one fatal flaw in Freudian theory when he says that "the psychoanalytic option holds an odd position, in a scientific sense, precisely vis-à-vis those colleagues who deny or even combat such an offending realization" (that humans are uncontrollably influenced by unconscious "wishes, fantasies, and dreams"). Those who dispute psychoanalytic interpretations do so for psychoanalytic reasons that are evident to the analyst but repressed by the critics. Therefore, those who argue for psychoanalysis support it (directly), and those who argue against it support it (indirectly). As tidy and gratifying as it must be to have such a universal solvent ready for one's opposition, this is precisely one of the major philosophical arguments that has led to the abandonment of Freudian psychoanalysis in contemporary science and medicine: it cannot be disconfirmed. Every bit of evidence—every bit one can even imagine—fits the theory with ease. For example, Jeggle claims that Freud has a simple interpretation of dreams: all geese dream of corn. That's an assertion one could test (not among geese, because of the interviewing problem!) among human dreamers, recognizing the corn as a metaphor for wish fulfillment. But that hope of testing disappears when we realize that when geese dream of corn, the corn is likely to be presented in the form of a ravenous wolf—or practically anything else—as a disguise. Then "corn" simply becomes the Freudian name for "the content of goose dreams." If we could find a way to interview geese, would we find that they dream of corn? Of course we would, if by "corn" we mean

all the things that geese dream of. What else would a goose dream of other than that of which geese dream?

In this connection Jeggle snidely remarks that “today’s sciences of man . . . regard themselves so highly” and then comments that those sciences “generally deny the unconscious.” This statement is not true and simply misunderstands (or misrepresents) the modern scientific view. The existence of mental processes that are not present in consciousness is undeniable (and did *not* originate with Freud). The issue is the typical use of “the unconscious” to mean the specific Freudian construction—one that *is* dismissed by most contemporary neuroscientists and dream researchers. But if Jeggle’s interest is in the conscious-unconscious boundary, then he should consider modern work on dissociation and the concept of cognitive operators (or “homunculi”) in neuroscience. These are all about non-conscious mental activity. In fact, in some cognitive science views, processes outside consciousness are more autonomous and powerful than in Freudian theory. In some, the idea of an acting consciousness almost evaporates into epiphenomenalism. (Not a position I favor!) There is plenty to debate in these fields, and much that I disagree with, but the ideas contested are based on solid empirical data and are clearly articulated. They are also relevant to modern efforts to understand human behavior, which makes them a more fruitful area of discussion than Freudian theory. Furthermore, if there are places where Freudian theory turns out to have empirical support, these approaches are better qualified to find that support than is conventional Freudian interpretation.

Jeggle grants that the “term ‘superstition’ has fallen into disrepute in folklore” (like Freudian psychoanalysis in medicine). He then provides an illustration of why, in my opinion, this is a good thing. If *superstition* has any usefulness at all, except as a handy way of insulting beliefs one does not share, it is to refer to those odd practices that people under stress acquire in an attempt to control the future that even they recognize as unfounded: the lucky pen used in exams, the non-religious athlete who crosses himself at the foul line or at bat. Jeggle links these to obsessive compulsive disorder (OCD), and I would agree that these behaviors do share something with OCD. But Jeggle goes on to make two moves that are common in this kind of psychodynamic speculation and that overwhelm the usefulness of the connection observed.

(1) In the first two paragraphs of the section entitled "Madness," Jeggle uses the terms *psychosis* and *neurosis* as if they were interchangeable, which they are not. Even Freud eventually realized that psychoanalysis had much less leverage with psychosis than with neurosis, and modern psychiatry has given up the psychodynamic explanation of such brain disorders as schizophrenia—explanations that burdened generations of women with the notion of the "schizophrenogenic mother." Yet in an earlier section Jeggle places superstition right at the boundary between "stubbornness of mind and *idée fixe*" (the fixed ideas characteristic of obsessional states) and then citing Binswanger's 1956 work goes on to suggest a dynamic range from normalcy through eccentricity to schizophrenia. In the "Madness" section Jeggle also repeats the Freudian aphorism that "patients love their madness." This is not my experience of people with psychotic illness, nor is it the view of modern psychiatry concerning major psychiatric illnesses. It is a terrible, frightening, and destructive thing to have a psychotic illness. People do not remain psychotic because they refuse to "give up" their illnesses, any more than people with cancer cling to their metastases (a view that some extreme versions of the alternative cancer approach of visualization have borrowed from Freudian theory). Even short of psychosis, acceptance of the "patients love their illness" dictum leads to some very cruel treatment of people suffering from depression.

(2) Jeggle begins with a reasonably narrow, if implicit, definition of superstition, linking it to compulsively repeated acts. We have some reason to worry, though, when he equates it with folk belief in general as well as with that "small madness we call superstitions or folk belief." We might take a little hope from the fact that Jeggle calls superstition a "religious substitute" and "a private faith that stands in close relation to pseudo-religious rituals." But later he connects it to "magical practices" in general (*magic* is a *very* problematic word that requires careful definition but does not receive it here) and still later notes "how closely connected religion is to superstitious practices." He goes on to grant that he considers prayers to St. Christopher by travelers as only "marginally" superstitious. (So they *are* superstitious, but some unspecified factor makes them less superstitious than some other unspecified practices. Superstition has degrees, but the metrics are not mentioned.) Jeggle says that all such beliefs and practices are involved, "following Freud, in the construction of a transcendental

reality.” Jeggle is “following Freud” here, accepting Freud’s construction of religion as a reality-distorting (i.e., neurotic) illusion, and now linking religion to superstition. This is a very common move among those who employ *superstition* to describe spiritual beliefs they don’t like. Beginning with unreasoned compulsive acts the term is expanded to include all sorts of things that the analyst considers irrational. Jeggle is not alone in this move. It is, in fact, quite common in commentaries on folk belief and religion, regardless of whether or not the scholar is a Freudian. A good example is provided by a recent (1997) book from Oxford University Press, *Believing in Magic: The Psychology of Superstition*, by psychologist Stuart A. Vyse. Vyse begins with typical compulsive rituals of athletes, but by the time he attempts an actual definition of superstition some nineteen pages later, he has added the following examples among others: beliefs in ESP, in angels, in miracles, in ghosts, and in reincarnation. After classifying all such beliefs as “irrational,” Vyse then presents the following as his actual definition (quoted from a 1956 essay by psychiatrist Judd Marmor): “beliefs or practices groundless in themselves and inconsistent with the degree of enlightenment reached by the community to which one belongs.” Vyse’s borrowing represents the application of yet another “discontinued model” to folk belief, and in this case by a psychologist who mentions Freud only once in a footnote and not at all in his bibliography.

So the problem here is not psychoanalysis. It is an epistemological issue that goes much deeper and that in Jeggle’s case is aggravated by the use of Freudian theory with its dogmas regarding spiritual beliefs. This radical inflation of a stigmatized term is a common rhetorical turn, but it is also an abuse of whatever term is being discussed. Technically, this is *equivocation*, a classic type of informal fallacy. It would be more straightforward simply to take the old theological usage in which superstition refers to idolatry (which is then applied to all religions other than one’s own). But all this loses the only useful distinction in the narrow meaning of superstition, *the unreasoned nature of the repeated act intended to control the future*. Doctors make (at least they *should make*) hand washing in clinics a ritual practice. It is done obsessively, and it is intended to control the future. We don’t call it superstition, but why don’t we? A bad reason would be because we believe in the theory that underlies the practice: that germs cause disease and hand washing removes them. That’s a sound theory but a bad criterion for defining superstition. Such criteria—do I share the

belief in question?—are what make the prayers of people in other religions superstition. The good reason would be that the doctor has and understands reasons for the compulsive act. The same might be said of fastening seat belts before driving. It is acting on reasons—rationality—that is the distinction, not whether we consider the conclusions of that reason to be true. Without refining this distinction, the definition is based on our own metaphysical assumptions. So what about the athlete who crosses herself before taking a foul shot? If she lacks religious beliefs but does this anyway, it seems to count as superstition in the narrow sense. If she *is* a devout Catholic (or high church Episcopalian, or anyone for whom crossing oneself is proper to prayer) and prays to be able to do her best, then she has reasons. Therefore she is thinking and doing something qualitatively different from the non-religious player, and we call her act superstition at our peril. The peril is two-fold: we lose the precision of the category that we find in natural language, *and* we simply impose a particular theological view on what is supposed to be a scholarly term. But this perilous usage naturally follows from Freudian theory, wherein all religious belief is magical thinking.

Another advantage to retaining the narrow meaning of *superstition* is that it is readily explained by an interpretation offered by B. F. Skinner, the behaviorist. Skinner noted that sometimes a positive or negative reinforcement is accidentally paired with a response, a process called adventitious reinforcement. Under some circumstances this results in a conditioned response. Skinner showed that this behavior is common in non-human animals too, in his classic 1948 article "‘Superstition’ in the Pigeon." There he suggests that under certain circumstances pigeons develop compulsive rituals (repetitions of the behavior adventitiously paired with the reinforcement) "intended" to control the future (that is, to elicit the reinforcer again—usually a food pellet in the case of the pigeon). This seems a solid analogy to the narrow meaning of superstitious behavior and confirms that such behavior does not require a reasoned inference—that it is not rationally founded (pigeons are notoriously poor at making sound inferences). If the behavior manages to recruit additional reinforcement, a lasting pattern can be established. Another example is the student who did unexpectedly well on an exam when using a brand new pen. If he becomes superstitiously attached to the pen, it may give him the confidence to relax and do better on future

exams, reinforcing the impression that the pen is lucky. If this sounds familiar, it should. The adaptive aspect of the superstition explains why Dumbo's "magic feather" allowed him to fly! (Modern children's literature is loaded with this kind of ideological training that explains "superstitious beliefs" as naive misunderstandings. Both the TV show *Scooby Doo* and the movie of the same name employ this as their only plot device.) True superstitions have this quality of conditioned (and therefore unreasoned) learning. They also seem effective at reducing stress, and failure to act on them increases stress.

Of course there is still an interpretive problem: should the observer get to decide whether the reinforcement was actually adventitious? When scholars have this license to decide they almost always do so on the basis of looking at the behavior and asking themselves if it "makes sense" to them. They then come to one of two conclusions: (1) if it does not make sense to them, scholars generally assume that it is not based on rational grounds and consider it a superstition, or (2) if it *does* make sense to them (as in the doctors' hand washing), then it is *not* a superstition. I have held that this is always an invalid process. The only way to know why people do things is to ask them. Granted, one may then hear *post hoc* rationalizations, but that problem is to be solved by multiple questions, subjects, and time points, as whenever we attempt to understand the conscious thought of others. Here is another of the overwhelming biases of Freudian theory: the speed with which it dispenses with conscious statements offered by subjects—especially whenever the analyst disagrees with the subjects' views of the world—and immediately spins out plausible but untestable hypotheses about unconscious motivations which by definition cannot be rational.

There is another problem with Jeggle's view of superstition. He writes that superstition "is a child of the pre-scientific world." And yet all of the behaviors he discusses, both narrowly and broadly defined, are ubiquitous in the modern world. He says of misfortune that while "superstitious contemporaries see it as a blow of fate, enlightened university library users call it a time-consuming annoyance." At least his rather charming use of "enlightened university library users" to refer to the second group makes clear his assumptions about the elite intelligentsia. But there is copious data documenting the widespread prevalence of these behaviors among the college educated—presumably including those who have used the library. He says that "the supersti-

tious live in a particular world in which only 'the superstitious coin is valid.'" And yet both survey data and ethnography on this subject—when carried out in the general population—show that those who have superstitions also use rational techniques in much of their problem solving. Jeggle's assertions, if true, would support Freud's view of "superstition." But they are not true, at least not true in the United States. And even if they were true in Germany (which I doubt), the American data would greatly complicate the view that Jeggle seems to hold of superstition as archaic.

Jeggle writes that the "ideal path to the unconscious and into the life of drives is dream interpretation," and he finds dreams important to the understanding of folk beliefs. Unfortunately, like many who use Freudian dream interpretation, he seems unfamiliar with (or else dismisses) modern scientific knowledge of the subject. His use of the German dream survey is a case in point. The reported frequencies of dream topics can only refer to a small subset of dreams—those that are recalled and for which the memory is retained. Most dreams are not recalled on waking, and most recalled on waking are not retained for long—unless promptly recounted—because they are out of the ordinary memory consolidation loop. This issue of dream recall is crucial information even for one who wants to utilize the Freudian dream symbol system. Most contemporary sleep researchers would say that this has nothing to do with repression in the psychoanalytic sense, but it seems that a Freudian might want to argue the point. From the psychoanalytic perspective, those dreams *not* recalled without special efforts of some kind (as during analysis and free association) would seem most important. I would also note that Jeggle's elaborate interpretation of his own dream should be put in the context of research showing that commitment to particular psychological dream theories actually shapes one's dream patterns: Freudians have Freudian dreams, Jungians have Jungian dreams, and so forth. Jeggle comments on the arrogance of "today's sciences of man, that regard themselves so highly," having suggested that such scientists dismiss psychoanalysis without giving it a fair hearing. Jeggle's omission of modern dream research information, much of it directly relevant to his topics and not inherently incompatible with Freudian ideas, shows that the tendency to reject out of hand works both ways across the neuroscience-psychoanalysis divide!

Not knowing the current dream research literature prevents Jeggle from understanding the significance of the 22 percent in the dream survey who said they have dreamed of “immobility.” Given the nature of the survey it is clear that this is in large part a reference to sleep paralysis, a state often called “dreaming” by the subject, to avoid embarrassing questions. I say it is clear because (1) immobility is the central subjective feature of sleep paralysis, and (2) because in surveys documenting the prevalence of sleep paralysis the rate in the general population lies between about 17 percent and 22 percent. While there is room for some additional dreams in the 22 percent figure, the great majority must refer to sleep paralysis. I have written extensively on this state and its relation to folk belief (in *The Terror* and elsewhere). And long before I did, and before modern knowledge of “sleep paralysis,” Freud’s own student and biographer, Ernest Jones, wrote extensively on the topic from a strictly psychoanalytic view but with extensive discussions of folk belief (*On the Nightmare*, 1931). In *The Terror* I spent a good deal of effort arguing against the psychoanalytic interpretation of sleep paralysis, but whether one accepts my arguments or not, the phenomenon of sleep paralysis is obviously of great importance in many widespread folk beliefs. Here, too, a narrow focus on Freudian interpretation seems to go with a willingness to omit the rest of relevant knowledge.

In closing, my central problem with Utz Jeggle’s essay is not that he finds Freudian psychoanalytic theory compelling and I do not, nor is it that he finds most “supernatural” belief irrational and I do not. My problem with Jeggle is threefold:

- 1) He applies Freudian theories without offering strong empirical evidence or any kind of systematic method for testing the suitability of these theories.
- 2) He stigmatizes all sorts of folk belief and religious belief by association with psychopathology; then he not only fails to offer a strong defense of his interpretation but also ignores generations of relevant criticisms of this view. (For an extensive discussion of the historical and contemporary problems of psychopathologizing spiritual experience and belief, in both psychiatry and psychology, see Hufford 1985.)
- 3) He dismisses a large array of directly relevant research literature from anthropology, folklore, sleep research, experimental psychology, neuroscience, medicine, and modern clinical psychology, most of it without specific mention.

As I noted at the beginning of my response, these shortcomings are not limited to Jeggle's scholarship alone. Rather, the problem is that when it comes to folk belief, folk religion, and related topics, each of these problems recurs with depressing consistency. Not only Freudians, but also a great many followers of other interpretive schools, make remarkably similar problems in the study of belief—principally a lack of interdisciplinary scope and a lack of rigor in method—his essay will have contributed to intellectual progress. The study of folk belief must become fair, empirical, and rigorous. Otherwise, the lingering suspicion that the best application of psychoanalysis would be to academics themselves will remain. Perhaps Freudian interpretation could reveal the unconscious fears and desires of scholars, "enlightened university library users" to use Jeggle's phrase, that trigger such irrational reactions to the common beliefs of ordinary people.

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