

Hypnosis *and* Related States

GILL
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Psychoanalytic Studies in Regression

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The Authors

INTRODUCTION

When we first decided to present a summary of our experiences with hypnosis it seemed to us a sufficiently complex and long-term project. However, as we proceeded with this initially circumscribed job, it became increasingly clear that the task was necessarily even more complex than we had thought. It gradually seemed to us that the difficulties we had encountered both in conducting the study and in conceptualizing our observations demanded our adding, to our own specialized narrative, considerations of broader significance. We increasingly felt the necessity to work out a comprehensive theoretical framework within which to place our data and have abandoned our original intention of presenting a simple account of our own investigations. We have tried, rather, to present in condensed form a fair sampling of our actual experiences over a period of roughly ten years—and more important, the sense these experiences seem to make when placed alongside of data from other bordering areas—in a contemporary theoretical context.

We have taken it for granted that our reader is reasonably familiar both with the standard literature on hypnosis and with the basic theoretical premises of psychoanalysis. However, we have tried to present a sufficient sampling of our actual observations so that the material might be evaluated by others who hold a theoretical viewpoint different from the psychoanalytic.

Because some of what we have observed and thought has been published by us in scattered journals, we have omitted

details of our studies available elsewhere to the specialized reader. However, most of what we present in this volume we have never published in any form before.

In this introductory section, we will present first a narrative account of our explorations and then the framework which we have tried to work out for these data. It is in the nature of "telling a story"—even a research story—that one cannot avoid giving it a continuity and a kind of straight-line coherence that did not exist so sharply in fact. It is difficult, if not impossible, to give an account of all the many detours, dead ends, contradictions, confusions, and otherwise seemingly wasted hours. Suffice it to say that our study had no fewer of all of these than any other research, and more than many. We have tried to present some of our dead ends and detours, and have only indicated others.

At the outset, the prime aim of our research was essentially practical: the effort to find out whether, and in what circumstances, hypnosis could be used as a device to shorten the duration of psychotherapy. Inasmuch as we started on these investigations at the very outset of our careers as psychotherapists,¹ our efforts were colored by that peculiar mixture of boldness and defensiveness which so often characterizes the work of the novice. Our first joint paper, published in 1943, reflected this attitude. Entitled "The Treatment of a Case of Anxiety Hysteria by an Hypnotic Technique Employing Psychoanalytic Principles" (101), this paper presented a case of a civilian patient who, in a relatively short time, showed significant improvement via the use of a technique which combined hypnosis with "psychoanalytic principles."

Although, in retrospect, we are aware of how little this therapy approximated analytic work as it is usually understood, we did make an observation in this patient which later proved one of our most important springboards for the further exploration of what happens to the functioning of

¹ One of us (M. Brenman) was then a research associate, the other (M. M. Gill) a psychiatric resident at the Menninger Clinic.

the ego in hypnosis, namely the fact of the constant fluctuation between a highly archaic, even primitive form of functioning and the maintenance of normal ego function. We did not begin to pursue this issue systematically, however, until two years later.

In the meantime, we devoted ourselves mainly to the further exploration of the use of hypnosis to abbreviate psychotherapy, most especially in the psychiatric casualties of war, at that time a problem of the highest practical importance. We worked simultaneously with war veterans² and with civilian patients in a variety of ways which we will detail in the body of this book.

As we continued with our therapeutic investigations we were inevitably brought face to face with the fact that many of the patients with whom we wanted to try hypnosis were simply not hypnotizable; we began to chafe at the number of profitless hours we were spending trying to induce hypnosis in patients who remained impervious to all our attempts. It was the effort to cut down on what seemed then a prodigal waste of time that led to two major, and essentially practical, explorations.

First came the attempt to establish a screening procedure; in this we were successful: all patients being evaluated at the Menninger Clinic took part in a group hypnosis "test." One or the other of us—and later advanced residents whom we had trained—conducted a weekly session in which a group of from three to ten new patients participated. Only those who showed a positive response were worked with individually, unless we had some special reason to work investigatively with a patient who showed a poor response.

Secondly came the exploration of ways and means to increase hypnotizability; in this, we were largely unsuccessful. We varied many things: male or female hypnotist, relative

² This work was made possible by the active help of Drs. Karl Menninger and John Greist, at the installation earlier an army hospital called Winter General Hospital, and later Winter Veterans Hospital, in Topeka, Kansas.

experience with hypnosis, the manner of approach to the patient, the physical setting, the amount of immediate motivation, and others. None of these appeared to make any significant difference. If a patient started out as a poor hypnotic subject, he *usually* stayed that way. We will discuss some interesting exceptions later on.

Now we began on a quite different tack, namely, the use of drugs in the effort to deepen hypnosis. When our first try with sodium pentothal transformed an indifferent subject into a good one, we made thirty new attempts. All of them failed. Our single exploration of the use of scopolamine and chloral produced a transient psychotic episode, an experience disturbing enough to discourage further work along this line. Work with sodium amytal yielded nothing much either.³

As it became increasingly evident that a person's hypnotizability was, by and large, a relatively stable matter, we began to inquire seriously and more systematically into what might be the essential differences between a good and a poor hypnotic subject. Although we emerged from this long and laborious series of investigations with data of some interest, we were unable to pin down in any definitive way (that could allow for *individual* prediction) the critical differences between people who responded well and those who responded poorly.

At first, proceeding on the impression that there were more good subjects among normal people than among patients, we formed the general hypothesis that hypnotizability varies inversely with the degree of maladjustment. We compared then a group of normals with an extremely "sick" group and another less deeply disturbed. The initial results here were quite clear: the normals yielded the highest proportion of good hypnotic subjects, the "less sick" group the next highest, and the "sicker" group the least. The attempt to break down psychiatric syndromes within the patient group showed that

³ This investigation was carried out by Dr. Harry Rand at Winter Veterans Hospital.

although the hysterics were (like most patients), on the whole, not hypnotizable, 40 per cent of them were. This per cent was the highest of any single psychiatric syndrome and only somewhat less than in the normal population. The occasional schizophrenic who proved to be an excellent subject made it difficult for us to take much comfort from the fact that our hypothesis regarding the inverse relationship of hypnotizability and maladjustment had proved to be *generally* correct.⁴

We then proceeded to *qualitative* studies of good and poor subjects—using batteries of psychological tests, autobiographies, clinical interviews, and free-association interviews. Here we amassed the proverbial “closets-full” of data, again with some provocative and interesting results, but nothing conclusive. Probably our most suggestive data have come from the analysis of the meaning of hypnosis to patients in long-term therapy. Here again our conclusions are surrounded by reservations.

In retrospect, it appears to us that these studies of hypnotizability acted as a kind of bridge between our initially practical interest in finding suitable patients for a therapeutic short cut and what became the core of our later interest, namely, the nature of the hypnotic process and its relation to allied states. The therapeutic situation continued to be our major source of data, though not our exclusive one.

The main reason we felt it necessary to explore avenues outside of therapy issued directly from the overwhelming complexities inherent in the treatment situation. The effort, for example, to sort out the normal vicissitudes of a developing transference from those *specific* to hypnosis proved extremely difficult. Moreover, granted the success of this undertaking, what guarantee had we that it was indeed this constellation of purely *psychological* factors that accounted for the development of the hypnotic state?

⁴ Details of this study were reported in 1949 by Dr. Gerald Ehrenreich (52), who had carried it out under our direction.

As we were struggling with these issues, the provocative work of Kubie and Margolin appeared in print (145). Theirs was the first really systematic attempt to integrate physiological and psychological factors in a theory of hypnosis. Thus it was that concurrent with our collecting data from clinical sources, we began to pursue other approaches.

One of the first of these was via the encephalogram. With the active assistance of Drs. Henry Luster and Morton Basan, we commenced this work at Winter Veterans Hospital. The results, at first, were startling. In hypnotic regression to a period of childhood, we began to obtain high frequencies; on the induction of hypnosis (contrary to the consensus of the literature), we obtained curves very similar to physiological sleep! But our excitement soon gave way to disappointment: we shortly discovered that these revolutionary findings had been the result of a faulty apparatus. As soon as we had it repaired, our results were consistently negative.

Now, for a time, we backtracked from the pursuit of such gross physiological changes as the key to the nature of hypnosis and, with the collaboration of Dr. Roy Schafer,⁵ set up a small pilot study of an area which we regarded as lying possibly somewhere between physiology and psychology: ego functions like attention, concentration, speed of association. Our aim was the comparison of such functions in normal and hypnotic states. Our results here were again negative. We thought perhaps we were using insufficiently sensitive instruments for this comparison. It was at about this time that we heard of Dr. Ward Halstead's battery of tests which could distinguish between normal subjects and fliers suffering from (permanent or transient) organic damage as a result of cerebral anoxia—when the usual battery of psychological tests could *not* make this distinction. The use of these tests was then our next move. Again, the results were negative.

⁵ Dr. Schafer was then on the staff of the Psychology Department at the Menninger Clinic; he is currently chief of the Clinical Psychological Service at Yale University.

We reconsidered then our plan to obtain physiological measurements in and out of hypnosis and concluded that we needed the collaboration of experts. We were able to make arrangements with Drs. Chester Darrow and Charles Henry, then at the Institute for Juvenile Research in Chicago. After numerous group-hypnosis sessions, we selected sixteen top-notch hypnotic subjects and nine who were totally refractory. Each of these twenty-five subjects was then seen individually to confirm the level of their hypnotizability and to get some data on their personalities. All twenty-five were then studied with simultaneous recording of EEG, blood pressure, EKG, eye movement, and psychogalvanic reflex. After this exhaustive run-through which took an entire day for each, we again interviewed the subject for his account of his experience during the tests.

Except for two abstracts (44, 45), the results of this work (43, 46) have never been published, the reason for this being that our two collaborators differed significantly in their evaluation of the data. We, ourselves, could have no view of the matter. We report some of the general results of this long detour in Chapter 6, "Hypnosis, Sleep, Somnambulism, and Dream." This proved to be our last expedition aimed at establishing the physiological differences between normal and hypnotic states.

Luckily, we had distributed our research eggs in several baskets. An experimental study conducted under our supervision by Drs. William Finzer, David Hilger, and Louis Kaywin was set up, designed to explore further the findings of Farber and Fisher (60) who had reported the capacity of a person in hypnosis to translate the dreams of other people. Although we were unable to confirm this result, we did find something which tied in neatly with the kinds of considerations developing in the growing body of data from our clinical work, namely, the fact that the person in hypnosis appeared to have significantly readier access to *his own* unconscious conflicts than he did in the normal state. We will report this

investigation in some detail in our chapter dealing with hypnosis and psychotherapy.

Concurrent with all of this, we had continued our observations of the induction process and of the established hypnotic state in long-term analytically oriented therapy. It was actually from this clinical work that our most usable data emerged.

Looking back over our unpublished research reports, we find that even during the period when a therapeutic short cut was our major aim, we were trying to understand or at least to describe the hypnotic state. As we went along, we found ourselves separating our data into two major categories: in the first, we were amassing records of phenomena which showed in hypnosis a wide variety of changes in self-awareness, bodily experience, emotional expression, motility, and the nature of thinking. In 1946, we presented a paper at the American Orthopsychiatric meeting, written in collaboration with Dr. Frederick J. Hacker,⁶ entitled "Some General Characteristics of the Productions of Patients in Hypnosis." Although, in our text, we had tentatively put forth the hypothesis that all of these changes seemed to reflect a significant *alteration of ego-functioning* in hypnosis, we did not put this in the title until our discussant, Dr. Leo Bartemeier, suggested that we ought to do so. This paper was later published in the *Bulletin of the Menninger Clinic* under the title "Alterations in the State of the Ego in Hypnosis" (27).

In the second category we found ourselves collecting observations which appeared to reflect the significance of the personal—mainly unconscious—relationship between the hypnotic subject and the hypnotist: we labeled these our "transference" data. Under this heading we collected a series of observations of different kinds. For example, we noticed the fact that patients frequently reported *spontaneous* changes in their subjective experience of the depth of hyp-

⁶ Dr. Hacker was then a resident at the Menninger Clinic; at present he heads the Hacker Clinic in Los Angeles, California.

nosis; these might occur several times in the course of an hour. In addition, we gathered numerous reports from patients as well as nonpatients of the occurrence of spontaneous hypnotic states when not in the presence of the hypnotist. The effort to pin down the psychological setting in which these events occurred occupied us for a long time.

We also pursued the unconscious meaning of hypnosis in a variety of ways: we accumulated data from analytically trained hypnotists with regard to their own insights into the meaning of hypnosis; we tried to analyze systematically in several long-term therapeutic cases the unconscious significance of hypnosis; we attempted to systematize our observations of a spontaneous regression in hypnosis (100). We will discuss later the details of all of these data. For now, we mean only to focus on the fact that for many years we found ourselves accumulating two apparently independent bodies of data from our observations of the hypnotic state, but were unable to discern any theoretical bridge between them. The observations of "altered ego function" and of "transference phenomena" seemed to us to be in quite separate realms of discourse. It was only in the preparation of this volume that we worked out a theoretical approach which may encompass both sets of data. In part, the development of this framework emerged as an outcome of our effort to sort out the similarities and differences between hypnosis and allied states, our data on the latter being largely restricted to reports in the literature. We found that reports on such diverse phenomena as, e.g., "brain-washing," presleep states, trance dancing in Bali, and the so-called "isolation" experiments frequently included observations which were in important ways very close to ours of hypnosis—though not identical.

Gradually, from a survey of all of these data together with an exploration of recent developments in ego psychology, we emerged with the basic theoretical premise of this book: *Hypnosis is a particular kind of regressive process which may be initiated either by sensorimotor-ideational deprivation or by*

the stimulation of an archaic relationship to the hypnotist. Here, then, was the link between our two sets of data. It appears to us that when the regressive process has been set into motion by *either one* of these two kinds of factors, phenomena characteristic of the other kind begin to emerge. Thus, for example, a prisoner in isolation deprived of his usual means of maintaining his hold on external reality may develop an intense, irrational set of feelings toward his jailor. Conversely, a psychoanalytic patient, in whom an ancient set of yearnings has just been released, may begin to develop bizarre bodily sensations. In hypnosis, as we will see, the attack is usually twofold and therefore more difficult to separate: the hypnotist attempts to initiate the regressive process by simultaneously stimulating intense infantile impulses and by systematic techniques of sensorimotor and ideational deprivation. When we began to consider our material in this way, we found that we were now free of the dilemma which had plagued us throughout this work: do we place the sensorimotor manipulations of the hypnotist—and the resultant changes in ego function—at the center of a theory of hypnosis, or do we give the prime theoretical importance to the revival of an archaic human relationship, i.e., “transference”? It now became clear that we do neither. Instead, the central position is occupied by the notion that the ego can become unseated, as it were, by one or the other of the two major attacks described, and that both sets of phenomena we had observed stemmed from this fact, whatever had initiated the loss of command usually held by the ego in a normal state.

In the series of related states where a regressive process is involved, each state requires its own “something more” than the unseating of the ego to achieve its own characteristic form. We would say therefore that whereas the “brown study,” the presleep state, or the creative burst are like hypnosis in that they all include some form of regression, that which distinguishes one of them from another lies in the

particular organization of each. We try to delineate some of these in Part II.

Before we close this introduction with a summary of contents, we will stop briefly to comment on the relationship of the view of hypnosis which we offer to the history of theories of hypnosis. The sharp conflict which has long existed between academic psychologists and psychoanalysts with regard to a theory of hypnosis represents, in our opinion, a minor skirmish in the struggle to lay claim to a systematization of the entire range of psychological function. The history of successively held theories of hypnosis reflects directly the prevailing attempts to establish a *general* framework within which to conceptualize all psychic activity.

On the simplest and most schematic level: sharp controversies have developed between those who are trying to build a general science of psychology based on some form of learning theory and those who base their theoretical formulations on some variety of psychoanalytic theory. In recent years, there have been attempts to build bridges from each side; the work of Tolman, Hilgard, Dollard, and Mowrer, for example, represents such an attempt from the side of the learning theorists. On the other side stands the work of Hartmann, Kris, Loewenstein, and Rapaport.

There can be no question that the abyss which used to separate the academic psychologist from the psychoanalyst is narrowing. There was a time when there was little or no overlap even in the content of the problems judged worthy of discussion. This is no longer true. Problems of unconscious motivation in human relationships, for instance, are no longer the concern of the psychoanalyst alone; nor are "sensorimotor" problems the private province of the learning theorists.

On both sides theoreticians are beginning to see as a proper area of inquiry problems which they have hitherto neglected, or even disdained. Such neglect or disdain is inevitable when the theory has no place for a variety of empirical observa-

tions. Thus as long as psychoanalysis, for example, remained a theory which revolved almost exclusively around “instincts and their vicissitudes,” it was impossible to construct a *psychoanalytic* theory of hypnosis, or of anything else for that matter, that would transcend these limits. We are taking advantage in this book of the fact that times have changed, and we present a theory of hypnosis which pays heed to the changes.

Specifically: before the expansion of ego psychology, the implicit assumption in psychoanalytic theory-building was that all human development is a *direct* outgrowth of “instinctual drives.” Functions like perception or motility were by and large never mentioned.⁷ Early, the search for repressed memories was the focus. Later, problems of instinctual conflict—in one form or another—became the central point of theory. Even the growing interest in the defense mechanisms did not substantially alter matters. It was thus not until Hartmann’s discussion in 1939 (114) of the apparatuses of the ego and their primary autonomy that room was made in psychoanalytic theory for the kind of consideration which had long been central for the “sensorimotor” theorists in the field of hypnosis. Kris’s (135, 136) concept of regression in the service of the ego provided us with a point of departure for our view of hypnosis as such a regression, and we will suggest that the central structural feature of such a regression is a subsystem in the ego. Hartmann’s (114) emphasis on adaptation and his concept of the relative autonomy of the ego from the id have been unified and broadened by Rapaport (189) into a general theory of the autonomy of the ego which he describes as relatively autonomous both from id and environment. We will propose a theory of hypnosis which leans heavily on the thesis of a diminution of these two relative autonomies. And lastly, we found Hart-

⁷ It is of incidental interest that Freud himself in his major theoretical statement, Chapter VII of *The Interpretation of Dreams* (76), did not omit a consideration of these problems.