The notion that hypnosis and hypnotic susceptibility are intimately related to the dynamics of ‘personality has been so much taken for granted for so long within the field that it may seem pretentious or trivial for anybody to try demonstrating the empirical relations between them at this late date. Since at least the time of Charcot and Janet, when hypnotic susceptibility was officially (and inaccurately) related to hysteria, the entire gamut of hypnotic phenomena has been casually associated to one or another aspect of personality or psychopathology, often without benefit of data. The result overall has been the gradual construction of a mythology about hypnosis and personality whose legends, if often based on truth, are sometimes also vague and contradictory.

Some such tales concern personality structure, relating hypnotic susceptibility in particular to more or less stable attributes of people. Included among these are the connection of susceptibility with hysterical personality, the idea that mentally retarded or psychotic people are particularly unsusceptible to hypnosis, or the more refined notion that neurotics are generally unsusceptible relative to normals, but that among neurotics, hysterics are more susceptible than others (Hilgard, 1965b).

Some stories about hypnosis and personality have a more dynamic bent and are more closely related to particular theories of hypnosis and general theories of personality than to the less elegant taxonomies of personality structure mentioned above. Perhaps the earliest such notion, following that of Mesmer’s “animal magnetism,” is that an ethereal quality called “rapport” develops between hypnotist and subject and permits all kinds of things to occur that could not happen without it. Modern versions of the rapport idea tend to be couched in the language of psychoanalytic theory, imputing the psychodynamics of transference to the hypnotic encounter, or else in the more general language of social psychology, of small group dynamics, or of interpersonal relationships. In all these genres, the dynamic messages seem to reduce to the idea either that the subject undergoes “a regression in service of the ego” or else takes on roles which are mutually and implicitly agreed upon between himself and the hypnotist.

Somewhat surprisingly, very little of the speculation about hypnosis and personality has a genetic or developmental character, that is, talks about the expression of hypnotic susceptibility or its inhibition in terms of the psychological events.
of childhood and of growth which might contribute to it. Since almost everybody believes that susceptibility develops with age to a peak around early adolescence and declines to some stable level in adulthood, implying that it changes with changes in personality development, it is all the more surprising that there has been so little discussion of the genetics of hypnotic abilities in the development of personality.

At all events, there has not been much. Among current students, Hilgard is almost alone in emphasizing the importance of studying genetic aspects of hypnosis. In his recent review of research in the field, he says: “It is plausible that both the abilities related to hypnosis and the relevant attitudes may develop in childhood, particularly in relation to parental figures” (1965a). He describes hypnotic susceptibility in terms of theories which emphasize its “ability component,” and says: “Where does this ability come from? To answer this question, reference is commonly made to native ability, learning, and socialization, but little direct evidence is available ... developmental-interactive theory has been proposed, which stresses early life experience as producing the abilities favorable to hypnosis.”

The object of my essay today is to comment on the relationship of personality to the development of hypnotic susceptibility by presenting data that bears directly on both. This paper surveys some of the early returns of a five-year study which attempted to examine the development of susceptibility as it occurred along with the personality events which might have most bearing on it. Though the study was initiated at the University of Illinois and all the data was collected there and at the Fels Institute of Research in Yellow Springs, Ohio, much of the impetus for this project was the work of the Stanford University Laboratory of Hypnosis Research under the direction of E. R. Hilgard.

The general design of this study was: (1) to measure carefully the hypnotic susceptibility of a large sample of children of different ages, (2) to measure carefully the personality characteristics which seemed most likely to have any connection with it, and (3) to compare (1) and (2) to each other. Three conceptual and methodological obstacles immediately appeared: (1) We did not know how to measure children’s susceptibility, (2) we did not know what we meant by personality, and (3) we did not know how to compare things we could not define in the first place. The solutions we obtained for each of these problems may be seen variously as a demonstration of scientific pedantry and insensibility or as a reasonable approximation of the only possible objective attack which could be made with the technology now extant for the study of human behavior or as something in between. In any case, understanding them is vital to understanding both the accomplishments and limitations of this research.

Any research program which purports to take a big blast into the rough rock of personality and relate virtually anything to it must justify itself as an exercise in scientific method. This sentiment does not so much reflect my own theoretical bias about the nature of scientific inquiry in psychology as it does a practical conclusion drawn from the sad experience of previous researchers. It is generally fair to say in personality research that there tends to be some topsy-turvy relationship between the amount of information one goes for in a study and the number of important conclusions that can be drawn from it. It is always fair to say, moreover, that there is such a relationship between the imprecision of design and execution and the kind and quantum of useful conclusions that can be drawn. If clarity and precision cannot be depended on to produce valuable results, and they cannot, vagueness of purpose, disdain for the logic of design, and sloppy collection and manipulation of data can be thoroughly trusted to reduce any research to meaningless intellectual garbage. Since I believe that many of the results of this project are very meaningful and important, it is consequently necessary for me to justify its methods in some detail.
The first problem, collecting a large sample of children and measuring their susceptibility, was the smallest of the three. With the cooperation of the Urbana, Illinois public schools and a number of private citizens, over 400 children between 5 and 16 from over 180 families were made available for testing, and later on 54 children from the Fels Institute longitudinal research project on child development were also examined for susceptibility. The definition of susceptibility was operationalized in what is by now a familiar technique in hypnosis research, the development of a scale which permits an experimenter objectively to examine a subject’s responses to, a wide variety of hypnotic suggestions when these suggestions are made in a standard manner to all subjects. The Children's Hypnotic Susceptibility Scale (London, 1963) was constructed for this purpose. Based on the widely known Stanford Scales of Hypnotic Susceptibility developed by Andre Weitzenhoffer and E. R. Hilgard (1959, 1962), the Children’s Scale makes provision for scoring the movements and statements a child makes in response to hypnotic suggestions (Overt Behavior), the apparent sincerity of his response to each item (Subjective Involvement), and an overall estimate of his responsiveness which combines both features of his performance. Overt Behavior and Subjective Involvement are so highly correlated with each other (.65 to .94) that they may both be measures of the same thing, but we only learned about this possibility after collecting the data. The reliability of the Children’s Scale, measured in terms of retest, the intercorrelations of its two parts, and the consistency of score distributions among many experimenters, is satisfactorily high - so if, in total, it is not an entirely valid measure of children’s hypnotic susceptibility, still it is certainly a very dependable measure of something! The basic description and normative data on the scale are now available in several publications, so I will say no more about them here (London, 1962, 1965).

The problem of approaching the development of personality was not resolved so simply as was that of susceptibility. In the first place, there is no conventional definition of personality which satisfies all the students of it, if indeed there is a definition which satisfies anybody at all. Most of us have only, a vague notion of what we mean when we use the term, and careful scrutiny of our own usage would probably show that we mean different things at different times and nothing at all sometimes. Personality theorists are not much help here either. In some respects, their many uses of the term all touch on common meanings which any thoughtful student of human behavior might concede is relevant to it. Whatever is meant by personality, it is agreed, the term implies:

a) some dependable characteristics of human behavior which
b) differ in some measure from one person to another, and
c) which are presumably subject to change in the manner of their expression as a result of growth and, in particular, of experience.

Such common aspects of definition for personality may sound wise, but they are not really very useful. In fact, skin color, height and weight, and food preferences are all characterized by all three attributes I have named, and if all of them are to be considered aspects of personality, then the term is so inclusive as to be meaningless. Perhaps it is!

The precise definition one gives to the idea of personality, in any case, is probably less important than the precise means he has of measuring it. If his instruments are very good, both in the sense that they can be depended on and that they are reasonably useful for predicting the relationships between the things they measure, then their careful use will actually help to define the concept at issue. If they are undependable, on the other hand, then it is meaningless to use them no matter how well defined the concept of interest seems to be.
Laying aside for now the problem of what is meant by personality, there are only a few kinds of tools available for its scientific study. Cronbach (1960) specifies three kinds of personality measuring devices: (1) Self-reports, in which the personality descriptions to be made about a person depend on things he has said about himself, (2) Judgments and systematic observations, in which conclusions about personality are drawn from things other people say about him, and (3) performance tests, in which inferences about personality depend upon things a person has done rather than tales which he or others have told about himself. Personality inventories like the MMPI, the Edwards Personal Preference Schedule, or the Guilford-Zimmerman Temperament Survey, are typical self-report devices. Sociometric ratings, case history reports, and Gesell developmental schedules all typify judgments and systematic observations. Tests of perception, motor skills, problem solving, memory and, to some extent, projective tests like the Rorschach or Thematic Apperception Test, are all typical performance measures of personality.

As must now be apparent, we did not approach this study by first defining personality at large, but by first asking ourselves what aspects of behavior and development might have plausible relationships to susceptibility and then seeking tools to measure those things. In some instances, as in the study of role-playing and susceptibility (Bowers & London, 1965; Madsen & London, In Press), we found it necessary to invent the tools, but for the most part we ended up following Herbert Muller’s parody that “invention is the mother of necessity” and fashioned our questions around instruments already designed to provide answers to them (Muller, 1952).

Each type of measure Cronbach describes has some merits and some disadvantages. We made virtually no use of self-report information for three reasons: (1) first, because previous work had turned up nothing of importance using this kind of information for studies of adult susceptibility and personality; (2) second, because it is almost impossible to find a self-report system equally applicable to the wide age range of children we were studying; (3) and third, because our original pilot interviews of children indicated that they were so variable in their ability to articulate their experiences that we would have exorbitant difficulty quantifying self-report data.

All our data was obtained from performance tests and judgments and observations. The virtue of performance tests is their relative precision of information and high reliability; their disadvantage is their narrow band width, that is, they tell more about less than other tests might. Our motives for selecting the particular ones we used where their plausible connection with hypnotic susceptibility; the tests included five measures of Intelligence (some brief verbal and motor measures, some Binet and WISC scales), two tests of Role-playing ability (including one of dramatic acting and another of the ability to simulate hypnosis itself), and eight measures of different modes of thinking, (including some Fels tests, a picture discrimination task, and some variants of familiar tests of mental set). Different combinations of these different tests were used for different individual experiments with different samples of children. The upshot of them all, however, insofar as they have been analyzed at this time, is that very few relationships between performance on them and susceptibility are significant; what is significant is subtle. Tests of verbal and motor performance which have successfully separated high- and low-susceptible groups of adults have not yet been tried on children, but the data from the children’s tests we have used is not encouraging. So much for performance tests in this study.

The main focus of our interest in personality development was on things like the relevance of social maturity and family and peer relationships, by way of interpersonal
variables, and imaginative activities, by way of intrapersonal variables each measured against susceptibility. Our main instruments for this purpose, consistent with an important trend in child development research, if not in hypnosis research, were observations of others. There were many of these, falling in two classes: (1) Standardized material pre-tested elsewhere and of known reliability, such as rating scales, and (2) general questionnaire and interview data derived from hither and yon.

The basic material for this part study is actually interview data, and the standard scales which were used, including the Vineland Social Maturity Scale, the Peterson Behavior Rating Schedule, the Parent Behavior Rating Scales (Baldwin, Kalhorn & Breese, 1949), Fels Day Camp Rating Scales, and Revised Fels Q Sort, may all properly be viewed as nothing more than systematic means of assembling interview materials so they can be scored easily and objectively. In addition to those materials, we also had available on the Fels children what is called “Black Book” data, consisting largely the detailed reports of home interviews conducted by trained social workers at least once yearly. For the Illinois children, moreover, we constructed personality questionnaires and interviews, taking questions of apparent relevance from revisions of the interview schedules reported by Sears, Maccoby, & Levin in *Patterns of Child Rearing* (1957) from the interviews that Hilgard’s laboratory conducted with college students concerning childhood experiences that might be related to hypnotic susceptibility, and from the Personal Experiences Questionnaires which Ronald Shor (1962) and Arvid Aas (1963) independently developed to relate hypnotic susceptibility to nonhypnotic experiences of everyday life. Perhaps it goes without saying at this point that we also collected vital statistics on sex, age, birth order, socio-economic characteristics, and the like.

By this time, some of you may have come to the reasoned conclusion that my intent in reporting all this is to induce a hypnotic state in the audience, but that is not the case at all. It is rather to impress you with the pains we took in this project to identify significant variables of personality development that would be related to hypnotic susceptibility among several samples of children and parents, a couple dozen instruments and several hundred items constructed and administered in a variety of ways by many observers with a variety of specialized training in observations or specialized knowledge of the children in question. The upshot of all this in brief, is that none of our instruments or items, taken alone, yielded any dramatic relationships, and few even demonstrated many statistically significant ones. The Peterson Behavior Rating Schedule, for instance, measures two traits: Adjustment - Maladjustment and Introversion-Extraversion, either of which might be plausibly related to susceptibility (1960). The test has been used previously with some success on school children with ratings made by their teachers. We used it with even greater confidence for ratings made independently by mothers and fathers, who should certainly know their own children better than schoolteachers do, especially with respect to traits like Patience and Impatience, Proneness to worry versus being Happy-go-lucky, or Cheerfulness versus being morose, three of the twenty items that make up the test. The result: correlation of both mothers and fathers of either Adjustment or Extraversion with any of the scores on the Children’s Scale range from -.01 to plus .16, averaging essentially zero for parent samples of 137 and more.

One could argue here, I suppose, that parents are insufficiently trained observers to obtain suitable ratings even on a well-developed instrument. In that event, one might reasonably argue also that the Fels Institute social workers, however well trained, could not make very useful observations for our purposes without more systematic instruments than they had. Indeed, we could not make quantitative head or tail of the very interesting
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information they contained. Here are some independent comments of two research assistants who tried independent means of summarizing and interpreting Black Book data:

VSL: “I have underlined in red several comments on sociability, dependence on other kids’ company, etc. At first, it seemed to show some meaning, but now ignore it—for these four subjects, anyway, it’s as vague as everything else.” The four subjects in question were respectively the two highest and two lowest-scoring children on susceptibility for the entire Fels sample.

Another said (CM): “The Black Book contained prose reports of home visits... appeared to be grouped into categories which appeared to lend themselves readily to quantification. The information was so varied and so unsystematic that for our purposes it was useless. The procedure I used in an attempt to rate the data was to listen to the tape while attempting to rate as many of the scales as possible. The scales were developed to be used in conjunction with an (independent) experiment I ran ... the only reason I sent you all the information was to indicate the extent to which we went to attempt to get something out of those books; however, when we are thinking of something as specific as hypnotic susceptibility compared with something so gross as some of the ratings, it’s bound to be a question.”

Perhaps the only way to get meaningful interview data for our purposes is to use both well-trained observers and carefully developed instruments, leaving as little as possible to the vagaries either of the items or the respondents. This is certainly a sensible notion, and indeed, it looks like the Fels data may yield a considerable number of significant findings in relation to susceptibility from nursery and day camp observations taken systematically by professional personnel.

On the other hand, some of the Illinois data call that very notion into serious doubt, for the material culled there from parents about personality development was divided into two parts: a questionnaire which parents filled out themselves and a subsequent interview administered by graduate clinical psychology students; each instrument contained some 50 odd items. In fact, the only intentional difference between questionnaire and interview items was that the former contained items that could be answered very briefly and clearly, while the latter had questions that required more elaboration. Thus the questionnaire may have been clearer and more reliable than the interview. In any case, when all the children were dichotomized into Low- or High-susceptible categories, based on Total Children’s Scale scores above or below 110, only five to eight questionnaire items had any significant relation to susceptibility and, still more disappointing, only three interview items were significant.

All of the analyses which have been described so far reflect the simplest possible examination of the data. It is certainly possible that with a more sophisticated approach to it, some more important and dramatic results may be observed. There is already some good preliminary evidence to this effect from a refined analysis of some of the data at hand - so let us look briefly at the phenomenon of Imaginative Behavior.

In the Illinois sample, parents were always telephoned very shortly after their child’s first susceptibility test and asked to answer some questions both about his reaction to the hypnosis session and his general behavior at home. In particular, the questions concentrated on his tendency to be imaginative and engage in an active fantasy life. Six items constituted the information elicited from parents about a child’s imagination: (1) Imaginary playmates, (2) whether he falls asleep easily, (3) whether he walks in his sleep, (4) whether he dreams much, (5) whether he talks to himself much and/or plays imaginative games alone, and (6) whether he plays imaginative games with other children. Now each of these items, taken by itself, has an extremely low relationship to susceptibility (−.11 to +.21 for N = 111). If all the six items
are pooled into an Imaginative Behavior score, moreover, it correlates -0.03 with IQ and only +0.11 with susceptibility, which is quite insignificant. On the other hand, the separate imaginative items are quite highly correlated with the total imaginative score (+0.35 to +0.66), and susceptibility has some low but significant correlations with several of the session report items, though not with Imaginative Behavior as such.

If the Imaginative Behavior score is broken down, however, and children classified on it as Low, Medium, and High, some very peculiar changes take place. For those 24 children low in imagination, the correlation of age and susceptibility rises to +0.57, though it was only +0.34 for the total sample of 111. For those 79 children rated medium on imagination, the correlation of age and susceptibility drops to 0.28; and for the eight children rated high in imagination, the correlation, though numerically 0.60, is altogether insignificant. This result apparently means that the relationship of age and susceptibility is colored by the imaginative capabilities of the child. Among relatively unimaginative children, susceptibility depends a good deal upon age; among those medium in imagination, it is less age dependent; and it is effectively unrelated to age among very imaginative children. Whether or not this result would be confirmed by a larger sample and a more concentrated examination of the moderating effect of imagination upon age and susceptibility remains to be seen; the point, at this point, however, is that the capacity of personality data such as that we have obtained to demonstrate any important relationships to hypnotic susceptibility seems to depend on more subtle manipulations of the data and more refined analyses than a superficial statistical examination provides.

**Summary and Conclusions**

That is not a hopeless, but neither is it a very happy result. Adding up the data on its face, we find that performance tests show few or only subtle relations to hypnotic susceptibility, a factored scale of adjustment and extraversion shows none, questionnaires and interviews taken in the laboratory show a very small return considering the size of the investment, the same kinds of materials taken in situ (Fels home visits) show no more, and the most promising kind of relationship, that of imagination and susceptibility, only becomes evident as a moderator variable. For any one of these samples or instruments, the fault might be said to lie in some error of measurement or observation, but for all of them to be erroneous in the same direction seems too much to swallow.

It is a lot easier to conclude that the straightforward assumption of straight-forward relationships between susceptibility and personality development is erroneous. Evidently, the hypnotic susceptibility of children is easier to identify than any of the things that determine it. Only when the possible determinants are teased and their facets exposed in multiple and subtle relationship to each other does their relevance begin to be seen.

The problem almost certainly does not lie in the definition of susceptibility or its measurement, but rather in the vagueness of our understanding of personality development and the impoverishment of our present means of measuring it. With the exception of tests of intelligence, and now perhaps of susceptibility, practically no measures of children’s personality development really predict anything worth knowing about the future of the children studied.

If there is a moral here for the scientist, it may be that he should repair to his laboratory to devise experiments for studying personality development rather than mere tests, for the former show more promise than the latter. If there is a moral for the clinician, it may be that he should temper some of his more glib propositions about personality development and mental health with the knowledge that he can hardly verify them. If there is a moral for the hypnotist, scientist or clinician, it is that most of the relations of hypnotic susceptibility to personality, in children, as in adults, still remain to be seen.
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References


Author’s Notes

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2 The research reported in this paper was accomplished with the support of the National Institute of Mental Health of the U.S. Public Health Service in the form of research grants MH-08598 (Development of Hypnotic Susceptibility, Perry London, Principal Investigator) to the University of Illinois and the University of Southern California and M-1260 (Lester Sontag, Principal Investigator) to the Fels Institute of Research.
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4 This idea may be the source of one of the parenthetical tales connected with hypnosis and personality, i.e. that the personality traits of the hypnotist are important to his operations. Whether true or not, this idea was more widely touted, if not believed, a couple of generations ago than it is now. Modern hypnotists seem to have discovered that respectability comes easier by being “just plain folks” or at least “just plain doctors” than by maintaining or claiming charismatic properties.