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The Prediction of Suicide

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CHAPTER ONE

Observations on Nosological Issues in the Classification of Suicidal Behavior

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THE BASIS FOR CLASSIFYING HUMAN BEHAVIOR

The classification of human behavior presents a major problem because no single criterion for classification exists.

When classifying the taste of substances, even their chemical structure is an unsatisfactory criterion, since there is little connection between chemical structure and taste. The search for tranquilizing drugs, for example, has revealed thus far that there is no way of telling the characteristics of a drug from its structure; a mere knowledge of a substance's structure does not enable us to develop better tranquilizers. What then is left for us to do when it comes to classifying human behavior?

Simpson's discussion of the science of "systematics" (Simpson 1961) provides a guideline to classification. The field of systematics refers to the scientific study of diversity among organisms, including not only the basic description of these organisms and their arrangements into suitable groupings, but also explanations of the causes and origins of these arrangements, including evolutionary mechanisms that have led to the observed diversity.

As far as human behavior is concerned, perhaps the approach of systematics with its stress on the explanations of the causes and origins is most satisfactory. The search for etiology in the classification of human behavior is similar to the search for evolutionary similarity in the classification of animals. However, there is much more known about evolution. Since we do not have any basic knowledge of the causes of behavior, all we can do is develop ideal etiologies through the use of scientific models; this would give us the structures from which to draw our hypotheses.

In searching for a group of scientific models that might be useful in explaining human behavior, the following come to mind: (1) the
ecological model, in which all of man's behavior can be related to the socio-cultural or ecological niche that he occupies; (2) the developmental model, in which man's behavior can be attributed to the variety of the developmental crises (critical periods) through which he passes and the proper satisfaction of needs at these junctions that may lead him in the direction of good or poor development; (3) the learning and conditioning model, which stipulates that man's behavior is primarily the result of the particular kinds and schedules of reinforcement to which he has been subjected; (4) the genetic model, which stipulates that man's behavior is primarily reducible to the genetic endowment with which he comes into the world; (5) the internal environment model, which stipulates that the body fluids and body chemistry are the chief bases of man's behavior; and (6) the neurophysiological model, which stipulates that man's behavior is to be sought in his neurophysiological equipment, especially the central nervous system. I have elsewhere applied these scientific models to the explanation of the etiology of mental disorders (Zubin 1969). Are they applicable to suicide?

There is one difficulty in all these classifications, arising from the essential fact that the classification of ignorance is always very difficult! But where our knowledge ends, our freedom to speculate or roam in the explanatory area is, of course, unlimited. Speculation is all that we can engage in at this point; controlled speculation may show the way for a type of research required for producing a suicide classification system of value.

WHY IS IT SO DIFFICULT TO DEAL WITH SUICIDE?

Suicide is the end result of a process, not the process itself. In most behavior disorders we have at least part of the process at hand for examination. In suicide, all we usually have is the end result, arrived at by a variety of paths. Unraveling the causes after the fact is well nigh impossible.

Since the completed suicides can be investigated only retrospectively, we are forced to utilize the unsuccessful suicides or suicide attempts for our source of data. But the unsuccessful suicides are no doubt quite different from the successful, and the former cannot be regarded as representative of the latter. However, there is some overlap between the unsuccessful and the successful, since some of unsuccessful try again and succeed. Thus the subset of suicide attempters who finally succeed provides us with the most valuable follow-up.

Even if we try to utilize the recidivists—those who attempt suicide one or more times before they finally succeed—as a basis for studying eventual completed suicides, they constitute only from 14–17 percent of those who eventually succeed, and that is indeed a small number out of the total that need to be followed up.

One of the primary considerations in suicide is intention. Intention alone is not sufficient if the act is unsuccessful, since doubt may be cast on the intention itself. Intentionality, a preoccupation of philosophical psychologists like Franz Brentano (Sullivan 1968), known more popularly as volition, the preoccupation of Narziss Ach (1935), has never recovered from the body blows that behaviorism gave it. Bumke's *Handbuch des Geisteskrankheiten* does have a chapter on the role of will in psychopathology, but it is limited to dissimulation of suicidal intentions (Bostroem, 1928). Until we revive investigations into intentionality and volition, studies of suicide will be hampered indeed.

Finally, before we begin looking at the scientific models for etiology, we need to resolve the question regarding the place of suicidal behavior in the entire spectrum of normal-abnormal behavior. This spectrum ranges from normality to deviant, disordered, of diseased behavior. Assuming that we can generally agree on what is normal behavior, although that is far from certain, we soon enter a nebulously defined region in which the overt behavior deviates from that which is expected in the particular ecological niche a person occupies in society, for example, delinquent behavior, criminal behavior, and sexually deviant behavior. Then there follows a region of disordered behavior in which the deviation is accompanied by self-reports of distress, that is, deviation from both local behavioral norms as well as from self-report norms, for example, neurotic depression, phobias, and character disorders. Finally, we enter into the region of disease in which in addition to deviation from expected norms in both overt behavior and in self-reports of distress, there are also anatomical and physiological deviations from expected structure and function. It is clear that suicidal behavior is at the very least a deviation from the expected in overt behavior, and at least in those cases that come to attention prior to the success.
ful act, a deviation from norms in self-report of distress. Whether or not it is a disease is not as clear, though suicides who have been declared psychotic either prior to the act or retrospectively may be regarded as diseased by those who regard psychosis as a disease.

It is interesting to note that the definition of suicide either as a behavioral deviation or as a disorder or disease has varied throughout time with the Zeitgeist—the trend of thought of the age (Rosen 1971). The ancient Hebrews recorded only five suicides in the Bible, and these all related to defeat in war; the only suicide in the New Testament is that of Judas Iscariot. The Hebrew word for suicide, which does not appear in the Bible but in later writings, is a clumsy combination of four words signifying one who destroys himself knowingly, again indicating that it was a rather unusual occurrence not much spoken of. It was not until the dawning of the Christian era that suicide became a problem, especially among the youth, but many of these suicides were not attributed to disorder or disease and appeared to be at worst deviant behaviors, often condoned by their cultures. There seemed to be at that time a search for the meaning of life, not unlike that which now pervades our youth, and for some, suicide was the answer. Thus, as a “social disease” it was tolerated and the situation did not change until Christian views began to influence social and legal attitudes. Up to that point, suicide was regarded as a deviant type of behavior. Of course, some cases of suicide by mentally disordered individuals were known in antiquity. It is probable that some suicide attempts were the result of pathological depressions, but the prevalent social attitudes led to the consideration of such cases as moral rather than psychopathological problems; they probably would have been classified in our terminology as disorders (deviations from social and personal norms). During the middle ages, suicide was regarded as a sin, and this view held implications for the soul and for property rights. Finally, with the coming of the age of enlightenment, the focus moved away from sin and its emphasis on a personal disorder, to the environment and its impersonal influence on behavior, thus returning to the realm of deviant behavior. The availability of statistics with the beginning of the nineteenth century (1820 and onward) made studies of the environment possible. Side by side with the statistical-ecological approach there also arose the medico-psychiatric approach, which stressed the endogenous forces in sui-

cide that arise from individual psychopathology. These two contrasting views still hold the field. It is quite likely that the etiology of suicide, like that of schizophrenia, covers a multitude of divergent causes, among which the ecological and the psychopathological are included, and this is one of the reasons for the slow progress in classification in this field.

WHAT ARE THE SCIENTIFIC MODELS FOR SUICIDAL BEHAVIOR?

In dealing with the ecological model, we shall focus on it as the primary etiological agent and consider the other etiological factors as nonoperative, even though this may strain our credulity. Our purpose is to see how far we can go with ecological explanations despite the probable multifactorial nature of the causes of the behavior disorder. We shall follow the same procedure in dealing with the other models. At the end of our discussion we will try to develop a supermodel—the epidemiological model—that will weight each of the component models and their interactions.

The Ecological Model

In examining the role of the ecological niche that a person occupies in relation to suicidal behavior, it becomes clear that this model has a longer history and is more highly sophisticated than the other models that have been proposed. Among the variety of formulations of the role of the ecological niche in suicide are Durkheim’s classic models (Durkheim 1951), Lindemann’s stress on the elicitation of suicidal behaviors in the vulnerable hyperergic (Gordon et al. 1950), Henry and Short’s model stressing the complementarity between suicide and homicide rates and relating the former to the absence of external restraints and the latter to their presence (Henry and Short 1954), and Cassel’s hypothesis regarding the role of poverty and privation (Cassel 1962 and Cassel and Tyroler 1961).

Perhaps the most prevalent model for suicide is the ecological model postulated first by Durkheim. According to Durkheim, the process leading to suicide is centered on a failure to accommodate oneself to the ecological niche one occupies, either because of too great acceptance or too little. Thus, altruistic suicide was the result
of too close an attachment, in which the individual martyrs himself for the socio-cultural code, as is the case with hara-kiri. Egoistic suicide is the result of rejection by society or inability to integrate with the social milieu, and anomic suicide is the result of self-isolation from society and its constraints.

Somewhat in contrast with Durkheim's focus on ecological stresses and strains in the production of high suicide rates, Henry and Short (1954) propose that suicide rates are higher when external restraints are weak and the individual must consequently bear the responsibility for the frustrations he encounters. In niches where the external restraints are great, homicide rather than suicide is the more common response to frustration. Thus, one would expect higher rates of suicide in social democracies and welfare states than in autocratic countries, more suicides in postpartums (after the stress is removed) than during pregnancy, and more in the congenitally blind whose defect is corrected. This intriguing hypothesis seems to require some experimental testing (Lester 1970).

Evidence for the tenability of Durkheim's hypothesis is amply illustrated in the rise of suicide rates during economic depressions, the decline during wars, and stability of national-cultural rates as long as their ecological niches remain stable. One social scientist, Cassel (Cassel 1962; Cassel and Tyroler 1961), has suggested that if he were given sufficient funds and assistance he could wipe out not only suicide, but also tuberculosis and schizophrenia in the deprived county in North Carolina where he worked, which was rife with these conditions. In fact, he regarded these three disorders as emanating from the same source of stress and was willing to accept a suggestion that this triumvirate of disorders be given a common name, schizotubercide!

Another hypothesis emanating from the ecological model is that of Lindemann (Gordon et al. 1950). He stipulated that hypereridism, a morbid state of hostile aggression, exists in some individuals who, in response to a series of repeated environmental provocations emanating from the ecological niche, turn their aggression inward, leading either to successful or unsuccessful attempts at suicide. By regarding suicide as the end result of a disease process, hypereridism, he revived and expanded Auenbrugger's conception (Rosen 1971) that suicide was the consequence of an emotional disorder, a "quiet rage" that brings on melancholy and eventually leads to self-destruction.

In order to derive suitable hypotheses for testing the tenability of the ecological model, it becomes necessary to develop a taxonomy of the ecological factors that may be important in the production of high suicide rates. At the present time, the factors of population density, socio-economic status, nutritional, educational, and cultural levels have been suspected as important factors. However, the multiplicity of such factors renders investigations almost hopeless, and unless we can systematically classify them, little progress in research can be anticipated.

It is rather discouraging to note that despite the evidence for the ecological model, little use is made of it clinically, and by the same token, sociologists and ecologists make little use of the clinical findings. It is well to bear in mind that in developing even first-order relationships between ecological factors, such as a comparison of rental rates in an area with suicide rates in the same area, two components of such relationships usually expressed as correlation coefficients, must be recognized. The total covariance between $X$, rental rate (average for the area) and $Y$, suicide rate for the area, can be separated into the following:

$$EXY_{(total)} = EXY_{(within)} + EXY_{(between)}$$

which can be readily transformed to the weighted sum of corresponding correlation coefficients multiplied by appropriate standard deviations.

Usually, sociologists and ecologists deal with the correlation between means of areas, neglecting the correlation within the areas. It can be demonstrated, however, that the total correlation will vary considerably depending upon the within-area correlations. This has been called the ecological fallacy (Zubin 1941). Clinicians, on the other hand, deal primarily with the within-area correlation, reflecting their knowledge of the individual patients they treat in the area; they are often ignorant of the correlation in areas other than those they are acquainted with. Furthermore, they are often ignorant of the correlation between the means of the areas. This is the clinical fallacy. A good example of such fallacious conclusions is afforded by the studies in the relation between schizophrenia rates and rental rates in Chicago (Faris and Dunham 1939). In subsequent investigations by Gerard and Houston (Gerard and Houston 1953) and Hare (Hare 1956) in which the records of schizophrenic individuals
were examined for family relationships, the negative correlation between rental and schizophrenia rates could be explained by the fact that most of the schizophrenics lived alone, apparently by personal preference either in flop houses or in plush hotels; since there were more "loners" in the low rental flop houses than in the plush hotels, the negative correlation between rental rates and schizophrenia is not surprising. It is clear that in order to test out the significance of the ecological variables, we need to consider clinical as well as ecological hypotheses and vice versa.

The factor of living alone or social isolation also plays an important role in suicide rates. It is interesting to note that according to Stengel (Stengel 1958), social isolation is one of the few dimensions that play the same causal role in attempted suicides as in completed suicides and that, therefore, social isolation ought to prove to be a good prognostic index.

Developmental Model

The most popular developmental model for suicide is the Freudian model. (I need only mention it here, since it is well known.) Evidence for the developmental model comes from the relation between early parental deprivation through death and subsequent suicide of offspring. Especially fascinating is the claim that suicides often are timed with anniversaries of fathers' deaths (Bunch and Barraclough 1971). Hendin's (Hendin 1964) hypotheses regarding the variety of child-rearing practices that differentiate the Swedes and the Danes with high suicide rates from the Norwegians with low suicide rates belong here. Unfortunately, a good deal of doubt has been cast on the validity of these rates.

It is interesting to examine, from the point of view of ethology, the "cry for help" that characterizes many suicide attempts. Is this a universal innate sign in man as is often the case in animals, or is it a culturally determined sign in only certain groups? It is, of course, regrettable that so few of the suicide attempters and those who finally succeed ever make use of the suicide prevention centers. This may indicate the cultural rather than biological origin of the "cry for help," and the fact that some members of our society, especially the suicide prone, never learn to seek help.

Another behavioral characteristic that develops with maturity is the perception of reward or reinforcement in life as contingent on one's own behavior or as independent of it (Rotter 1966). The belief that one can control or influence the environment with his actions, and that he himself is master of his fate to a large degree, characterizes those whose locus of control is designated as internal, while those who regard themselves as powerless pawns in the hands of fate or luck are regarded as having an external locus of control. As examples, we might take the young women who lead very independent lives with a definite career and have a high suicide rate and contrast them with the older women who have had relatively sheltered, dependent lives and also have a high suicide rate. It is possible that in both these groups, a shift in locus of control may have taken place that brought about the crisis leading to suicide.1 Thus, the young independent girl may have suddenly lost her inner locus of control and shifted to an external locus, which she finds herself powerless to cope with, thus the crisis leading to suicide was produced, and vice versa for the older woman.

In dealing with the loci of control we must remember that according to Rotter, it is the person's perception of the locus of control that matters, not the reality of the source of control. Thus, belief in internal control, like belief in freedom of the will, may be only a delusion, but it is the existence of this belief that makes the difference. Practically, however, we must realize that this belief may clash with reality, and then conflicts may ensue.

This conflict becomes especially pronounced when we combine the concept of locus of control with the concept of environmental freedom and constraint (Henry and Short 1954). According to Henry and Short, an environment with high constraint will produce high homicide rates while an environment with low constraint will produce high suicide rates. Let us see what effect loci of control may have when paired with degrees of environmental constraints as shown in Table 1-1.

Following Henry and Short's analysis we would expect conflict to arise when an internal locus is paired with high environmental constraint, thus producing a high homicide rate. When an internal locus is paired with low environmental constraint, a high suicide rate should follow, since when something goes wrong, one cannot blame

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1 I want to thank Dr. J. Diggory for the suggestion that the shift in locus rather than the locus itself was the probable causal factor.
Table 1-1

<table>
<thead>
<tr>
<th>Locus of Control</th>
<th>High</th>
<th>Low</th>
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<tbody>
<tr>
<td>Internal</td>
<td>High homicide rate</td>
<td>High suicide rate</td>
</tr>
<tr>
<td>External</td>
<td>Passivity</td>
<td>Escapism</td>
</tr>
</tbody>
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it on external constraints and must take the blame himself, with resulting feelings of guilt.

For the other two rubrics, Henry and Short make no predictions. But following along with their hypothesis, we might expect that when an external locus is paired with high environmental constraint, some type of passive adjustment will probably ensue, and when an external locus is paired with low environmental constraint a vacuum is produced, because there are no external guidelines for the external locus of control to follow. Confusion, aimlessness, and goalless activity may ensue that may open the gates to escapism into drugs. We must consider both the locus of control (clinical) as well as the constraints of the environment (ecological) in making predictions. How the suicide rate will fare for these two rubrics is not clear.

Learning Theory Model

The learning theory model stipulates that one learns to adopt suicidal behavior in the same way that he learns to adopt coping behavior. The occurrence of suicide through imitation or modeling and the hypothesis that somehow or other the would-be suicide cannot learn to adjust to life's frustrating vicissitudes are examples of the usefulness of this model.

One of the most intriguing hypotheses arising from the learning theory model is the possibility that, at least for some individuals, repeated suicide attempts fall into the same category of behavior as incomplete or interrupted activity, which gives rise to the Zeigarnik effect and to the compulsive necessity for completion (Zeigarnik 1927). Ach (Ach 1935) in his monumental book, Analyse des Wil- lens, has reported a series of experiments in which he examined the power of various substitute activities in releasing the tension of the interrupted activity. Perhaps completing the suicide attempt in videotape or in motion pictures and manipulating the result of the act to show desired or undesired effects on family, friends, and even on those whom the would-be suicide is trying to punish with his act might serve as a release and in this way eliminate further attempts. Resnik and his group at the National Naval Medical Center are working in this area (Resnik et al. 1972).

In contrast, to return to the notion of modeling, we might wish instead to present such an individual with demonstrations of alternative (nonsuicidal) conclusions to the events leading up to his previous suicide attempts. In line with the behavioral rehearsal techniques being used by some behavior therapists, we might even instruct the individual to perform these nonsuicidal alternatives after viewing them, and arrange for the therapist and perhaps family members also to deliver immediate positive reinforcement in the form of approval or tangible rewards. Frederick and Resnik have described such a clinical program (Frederick and Resnik 1971).

An interesting angle on interrupted activity as a source of suicide is provided by Bryan's study of high-class call girls (Bryan 1969). Fifty-five percent of his respondents indicated that they had, with serious intent, attempted suicide, but most attempts by the same girl were made prior to embarking on her call-girl activities. Although the data are meager and the sampling rather biased, one might suggest, perhaps with tongue in cheek, that the occupation may serve as a prophylactic against suicide, or is it perhaps a substitute activity?

One of the contributions of learning theory to the diagnosis of any disorder is the determination of the contingencies in which deviant behavior occurs and what maintains such behavior. Such behavioral analyses constitute the basis for behavior therapy. Target symptoms are found on which the efforts of the therapist can be focused. Similar analyses of contingencies that elicit suicidal behavior (including ideation) and those that maintain it can become a new direction for suicidal research.

In dealing with suicidal behavior in terms of reinforcement, the class of such behavior will probably have to include responses other than the suicide attempt itself. The chain or sequence of responses leading to the attempt may occur and be reinforced in other contexts as well, for example, behaviors such as talk of death, injury, weapons, accidents, unduly frequent exposure to hazardous situations, etc., could lead to a variety of reinforcements that, especially
most important to try to determine degree of intentionality in those who had made suicidal attempts in order to try to initiate preventive measures to alter their attitudes. The first steps in this direction have indeed been taken by Dr. Beck et al. in chapter three of this volume. He documents retrospectively all the events prior to suicide that could be interpreted as indications of intentionality. Similar data are obtained on unsuccessful suicide attempts. In contrast with Beck’s direct interview method, which, as he points out, depends upon circumstantial data in the first section of the interview and upon willingness to cooperate and confide in the second section, it is possible to utilize methods that may be relatively freer of the difficulties he mentions. Perhaps verbal reinforcement techniques might help to determine degree of intentionality. In the case of schizophrenic patients, it has proven possible to determine degree of affective capacity by means of verbal reinforcement (Salzinger and Pisoni 1958, 1961). Why should it not be possible to utilize the same technique for determining degree of intentionality? If it is feared that such reinforcement might be deleterious to the patient, one could reinforce only the more hopeful or benign statements regarding the future. The hypothesis would be that the suicide-prone with high intentionality would be less amenable to such reinforcement. The sudden drop off of benign statements regarding the future when reinforcement ceases could be used as an indication of high intentionality.

Genetic Model

Kallmann’s thorough search of the literature and subsequent searches have indicated that, excluding suicide pacts, only one or two instances of concordance with regard to suicide in identical twins have been reported (Kallmann and Anastasio 1946). It must be concluded that genetics, per se, either plays a small role, or the population required to test the hypothesis would have to be so tremendous, since the combined probability of being a twin and committing suicide is probably in the Poisson range, that our data are insufficient to test it. Familial occurrences of suicide are, of course, no evidence for genetic influence.

The fact that suicide is a rare phenomenon might lead one to assume that the genotype prone to suicide, if there is one, is very rare in the population.
The Internal Environment Model

According to Bunney et al. the level of urinary 17 hydroxycorticosteroids in both male and female patients who later tend to engage in suicidal behavior tended to be higher than in normals (Bunney et al. 1969). While the rise in level does not occur in all the suicide prone, nor is it specific to suicide, it does offer a basis for biochemical research that may uncover underlying bases for the suicidal behavior. However, preliminary reports do not confirm the initial study (Levy and Hansen 1969). Reserpine, and its relation to monoamine oxidase alteration and to suicidal behavior, is another problem calling for investigation.

Neurophysiological Model

There is no direct data on neurophysiological aspects of suicidal behavior, but there is one aspect of suicidal behavior that can be approached experimentally through neurophysiological considerations. The processing of information that goes on in the brain of the would-be suicide seems to indicate a tendency for him to be fixed on a course of self-destruction as if no other options were available. It seems as if he is reversing the steps in the search for identity that is typical of most youth and seems to find reasons, not for further search, but for giving up the search and regarding himself as irretrievably lost. He behaves like an animal in a ratio-strain experiment where the probability of obtaining reinforcement is so low that no variability in response occurs and in fact his entire repertoire of behavior is completely extinguished except for only one response—self destruction. How the options were eliminated and how they could possibly be reopened is indeed an interesting problem. An indication of how loss of freedom of choice affects behavior is offered in the following study.

A recent experiment by Glass et al. (1971) demonstrated what happens when no options are available in a stressful situation. Persons subjected to a noxious noise but provided with the option of turning off the noise subsequently fare much better (after the stressor is removed) in the processing of information than do similar individuals who have no such option and are forced to endure the stress without any recourse. The experiment was based on Selye’s notion that the “psychic” resources of an individual are depleted during the process of adaptation to a stressor, and this depletion leaves him less able to cope with subsequent environmental demands. Both groups had become adapted to the noxious noise, as demonstrated by the leveling off of their neurophysiological responses, but the price paid for this adaptation was greater in the group without options. The mere presence of the option, even if it is not used, makes for better performance in subsequent tasks. It is quite likely that the suicide-prone are in the same position as the individual in this experiment without an option, and even if they were provided with a button to eliminate the noise, their psychic loss would be greater than the nonsuicidally inclined or those less inclined in that direction. Perhaps such an experiment on unsuccessful suicides might prove valuable in distinguishing the high-intentioned from the low-intentioned individuals.

Thus far we have dealt with each model in isolation. It is clear, however, that none of these models operates alone. When we examine a given case, we can find evidence for the entire range of scientific models and indeed, in epidemiology, the entire range is actually examined and the relative importance of each model is noted. Thus, in the case of the mental disorder of neurosis, the learning theory model may be the most important while the ecological, genetic, and the like are only contributory. In the case of suicide, the relative weights of each of the component models is still to be evaluated. At the present time it seems that the ecological and developmental models seem to contribute most in the light of available data while the genetic and neurophysiological models seem to contribute least, with the internal environment and learning models somewhere in between.

The scientific models discussed above were focused on etiology. Of course, one could focus on treatment models, on actuarial models for prediction, or on forensic models, but it is likely that the etiological models would provide a scientific basis for most of the others.

Methodological Problems. The application of the descriptive approach of Pokorny in chapter two of this text and of the scientific models for etiology proposed in this paper present certain methodological problems. First is the problem of whether suicidal behavior is distributed continuously in the general population. Is it like convul
sive behavior, which may occur due to a disease process but may also occur as a result of environmental stresses like hyperventilation and electrical stimulation, or is it limited only to certain predisposed individuals? It may be safe to assume, for lack of better information, that the tendency towards suicide is distributed symmetrically, somewhat like intelligence in the general population, and that its distribution ranges from the extreme left, which represents the zero point of this suicidal tendency, to the extreme right, which represents successful suicides. The right end of the distribution may contain two groups, one group that is simply an extension of the normal distribution of the extremely suicide-prone, and another group that is catapulted into suicidal behavior either through environmental stresses, psychopathology, or perhaps even genetic predisposition. A similar bump in the end of the distribution of intelligence was found by Roberts (Roberts 1950) that was also composed of two groups: the naturally dumb, who belong in that portion of the curve as a result of polygenic inheritance, and another group who were catapulted into that region by traumatic or genetic disorders as evidenced by the fact that their siblings come from portions of the curve covering the entire range from low to high intelligence. But even if the successful suicides represent a totally different sample, that portion of the successful suicides who come to attention in their earlier unsuccessful attempts can still provide us some information on the eventually successful suicides. Perhaps the typological analyses now available for finding the clusters of individuals who are like-minded may help in separating the extreme group of the successful suicides from those who do not belong with them (Fleiss and Zubin 1969).

Another problem that we are faced with is the question of prognosis versus prevention. In accordance with our earlier discussion, individuals who cannot accept a patient role, whose locus of control suddenly shifts from internal to external control or vice versa, are at the highest risk of committing suicide. These characteristics, together with those described by other contributors in terms of ecological variables and age and sex, are the markers that are needed to locate the suicide prone. Elsewhere (Zubin 1970) I have pointed out that mental disorders are all time-limited and that with or without therapy they tend to be self-limited. In other words, all patients recover, but when they do recover, they return to their premorbid level. If they had a good premorbid personality (and if the disorder did not leave any disturbing residues) they tend to return to their premorbid adjustment and are regarded as improved or even "cured." If, on the other hand, they had a poor premorbid personality, even when the disorder is lifted, they still cannot cope and give no evidence of improvement. They are often regarded as still suffering with the disorder even though it has disappeared long ago, as is the case with the social breakdown syndrome (Gruenberg 1967).

Thus, the new function for therapy is not to "cure" the disorder, but instead to remodel the premorbid personality so as to enable it to cope once the disorder is lifted. This is especially applicable to the suicide attempters. While in the majority of cases the conditions leading to the suicide seem to disappear, in some cases repeated attempts occur. For such individuals removal of ecological strains, education in adopting the patient role, and examination of the locus of control and its stabilization are essential if future attempts are to be prevented (Zubin 1970).

Actuarial versus clinical prediction is another problem that has been raised. In an earlier discussion (Zubin 1955) I have pointed out that this is a pseudo problem, depending upon the state of knowledge and upon the skills of the clinician. A clinician who knows all that the actuary knows and has additional knowledge that the actuary has not yet incorporated will be the winner in a contest between the two, and vice versa. You can imagine that over the shoulder of the best actuary stands a clinician who absorbs all that the actuary knows and adds his additional information, but behind him stands another actuary who incorporates the new knowledge, and so on to an infinite progression. Perhaps, as the information gets more and more complicated, the excellent clinician will depend more and more on computers to give him the ad hoc information to which he
can add his new nuggets. The same argument can be applied to the continuity-discontinuity conflict. At the present time, our knowledge is insufficient for either assumption, but as knowledge increases, our faith will seesaw between the two, depending upon the available information.

A problem often arises regarding the utilization of less expensive interviewers to collect the information when highly skilled clinical interviewers are not available for field work: once the data are collected, the highly skilled clinicians are brought in to evaluate the data. In our own work we have depended only on highly trained clinicians to do the field work and it would be advisable that in the present state of knowledge regarding suicide, we should not depend on semiskilled clinicians. One example might suffice. In our study of community leaders in Washington Heights, when we presented the protocols to experienced clinicians without identifying whether the protocols were from patients or normals, most of our leaders, who showed considerable psychopathology in the systematic structured interviews, were diagnosed as sufficiently mentally ill to require hospitalization. However, our clinical interviewers were able to see enough of the personality assets of the individuals to realize that they functioned despite the presence of some psychopathology. It is notable that most of our instruments assess liabilities and leave assets out, a fault that needs remedying.

The problem of self-administering techniques versus interviewing is another area requiring further investigation. Certainly in the case of the suicidal who are psychotic or who are strong dissimulators, severe limitations are placed on self-reporting instruments. Only careful calibration of the patterns of responses on the self-reporting instruments against systematic structured and objective interviewing can resolve this issue (Zubin 1968).

Another issue that often arises concerns the development of measures of agreement between two raters. Often, the two raters are matched in a fourfold table and the value of $X^2$ for the fourfold table is computed as a measure of agreement. It must be recognized, however, that for the purposes of a measurement of agreement, Kappa is much superior (Fleiss, Cohen, and Everett 1969) because $X^2$ is a measure of association that is fed equally well by deviations from chance disagreement or from chance agreement. Kappa measures only deviations from chance agreement.

With regard to the role of mental disorders such as depression or schizophrenia, the newly developed systematic structured interviews for the presence of psychopathology ought to be of help (Zubin 1969).

Regarding the possibility that all suicides are mentally ill, it is of some interest to note that according to Robins (Robins et al. 1959), fully 94 percent of successful suicides in St. Louis were found to have been psychiatrically ill when detailed clinical information was obtained from relatives, only 4 percent were terminal medical cases, and only 2 percent appeared to be well. Similar findings have been reported by Barracough in a study conducted in England. It is interesting that during the seventeenth and eighteenth centuries the notion was widespread that England was the land of melancholia and suicide; such conditions were named “The English Malady.” It is indeed encouraging to note that despite this somber history, the suicide rates in England are dropping, as reported in Barracough’s paper. Whether one can safely assume, as the author does, that the drop is attributable to preventive treatment is still an open question until all other competing explanations are explored.

Durkheim in his studies took care to eliminate psychiatric cases from his data and even excluded Jews because of their alienation in those days, yet had left a substantial number of Protestant and Catholic suicides presumably free of mental disorder. It might be well to repeat the St. Louis study and the English study with a suitable control group who meet death partially through their own actions, albeit accidentally rather than intentionally, as might be afforded by accident mortality cases—for example, automobile fatalities. Though this source might not be entirely free of the suspicion of suicide, it would nevertheless provide a check on the question of whether the search for evidence for mental disorder that was undertaken in the postsuicide studies might not reveal as high a rate of mental disorder among the accident deaths as among the suicides. Perhaps even a clearer control group could be provided by considering only passengers’ deaths, excluding those of the drivers.

As for future attitudes of society towards suicide, especially in the wake of population pressure and of the prolongation of life by biomedical techniques, only time can tell. There is, however, a choice bit from Eugen Blueler (Blueler 1950) that I would like to call to your attention:
The most serious of all schizophrenic symptoms is the suicidal drive. I am even taking this opportunity to state clearly that our present-day social system demands great, and entirely inappropriate cruelty from the psychiatrist in this respect. People are being forced to continue to live a life that has become unbearable for them for valid reasons; this alone is bad enough. However, it is even worse, when life is made increasingly intolerable for these patients by using every means to subject them to constant humiliating surveillance. Most of our worst restraining measures would be unnecessary, if we were not duty-bound to preserve the patients' lives which, for them as well as for others, are only of negative value. If all this would, at least, serve some purpose! However, like Savage, I am convinced that in schizophrenia it is this very surveillance which awakes, increases, and maintains the suicidal drive. Only in exceptional cases would any of our patients commit suicide, if they were permitted to do as they wished. And even if a few more killed themselves, does this reason justify the fact that we torture hundreds of patients and aggravate their disease? At the present time, we psychiatrists are burdened with the tragic responsibility of obeying the cruel views of society; but it is our responsibility to do our utmost to bring about a change in these views in the near future.

DISCUSSION

The descriptive schema provided by Dr. Pokorny in the following chapter seems excellently suited to describing the area of discourse known as suicide. For scientific classification we need to go deeper into causes, and since causes are still unknown, we must use “as if” causes as scientific models. By providing us with hypotheses for further testing, these models will enhance our knowledge.

At the present time, the most promising individuals for investigation are the recidivists who eventually succeed, even though they constitute only a small portion of all the recidivists. The one outstanding prognostic feature for all suicides is the prior history of attempted suicide. As always, the past is the best prediction of the future.

Since the purpose of scientific models is to provide hypotheses for probing their tenability, what tools do we have and what tools need to be created for testing these hypotheses? Let us accept the assumption that the best population to study is the high risk population—the recidivists who contain as a subset the group that will eventually succeed in their final attempt (unless we find ways of preventing them).

For testing hypotheses emanating from the ecological model, we need to develop a taxonomy of the parameters that characterize the niche that the high risk group occupies.

For the developmental model, we need assessment of the subjects' life histories and how they underwent the critical stages of development. Here only interviewing of the group or prospective observations of families with high risk of suicide can be of help.

For the learning theory model, we need careful observational studies of the selected group to determine what situations tend to reinforce suicidal ideation and what maintains such behavior. Interviews with verbal reinforcement for suicidal ideation after various crucial experiences is one way of arriving at such information. For the genetic model, nothing much can now be suggested except to note whether the hereditary loading of some types of manic-depressives will distinguish the suicide prone from the suicide resistant. As far as the internal environment is concerned, laboratory tests following crucial experiences, which may elevate suicidal trends, may be in order. I have already indicated the type of experimentation that may be useful for testing the neurophysiological model.

What I am suggesting is to utilize a high risk population and push each of the models as far as we can to determine how much of the variance of suicide proneness can be explained by each separately, and then how much can be explained by their various interactions.

With such a program we can perhaps push forward the frontiers of knowledge in this baffling field, which has thus far yielded only to mass statistical prediction. It is recognized that only some 15 percent of the group we are to follow up will yield pay dirt in terms of our expectation, but a successful study of these 15 percent ought to prove very rewarding in prognosis as well as in prevention.

If we could combine a descriptive approach with the etiological models described here, research in suicide behavior might become more fruitful.

REFERENCES


