A Biometric Approach to the Classification of Suicidal Behavior

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Delivered before "Workshop on Measurement of Suicidal Behaviors", October 14, 15, 1971, Penn Center Inn, Philadelphia. This paper developed in response to a request for discussing a paper by Alex D. Pokorny on "A Scheme for Classifying Suicidal Behavior".

In approaching the problem of suicidal behavior it is, first, necessary to define it and to describe the territory that it covers. This has been done quite well by Dr. Pokorny and his committee and all I can do is marvel at its simplicity, complete coverage of the field, and delineation of its various aspects and subdivisions. Adopting his descriptive schema would do wonders for the literature in this field and if his definitions are accepted, make the results of future studies comparable. However, he has adapted a rather limited view of classification and it is my purpose to extend this view to cover a more catholic view of classification, especially from the biometric point of view. Biometrics deals with the classification and measurement of living organisms and here we are concerned with the classification and measurement of those behaviors of man which lead to self-destruction.

Let me say at the outset that what I am placing before you is well known to you. If it has any novelty, it is in the way I have organized the
old material and thus provided at least for myself a new way of looking at the phenomena which may engender new research.

I. What is meant by classification

You will recall that Plato and his disciple Aristotle laid down the following assumptions for a classification scheme: a universal order exists in nature which permits carving it at the joints into natural classes according to the similarities of the attributes which the natural phenomena possess.

Plato divided classification systems into two types: (1) classification based on visible things or their images and (2) classification based on intelligible concepts and their ideas. In Aristotle's hands these classification systems became a search for determining the primary essence of the phenomena under observation and once this essence was discovered, the phenomena could be pigeonholed into their natural class.

Linnaeus many centuries later implemented Aristotle's schema by devising a naturalist's approach for distinguishing 'the parts of natural bodies with his eyes, describing them appropriately according to their number, form, position and proportion and naming them'. In a sense Dr. Pokorny, too, has followed this schema, giving us a naturalistic description of suicide, by providing a description of the behavior, teasing out its components and naming them. To become the Linnaeus of suicide is no small contribution!

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1 This discussion is based on (Zubin, J., 1968)
The question arises, however, whether such classification is really the goal of science or merely propaedeutic to science.

Alfred North Whitehead (Whitehead, A. N., 1956) has this to say in answer to this question: "In a sense, Plato and Pythagoras stand nearer to modern physical science than does Aristotle. The former were mathematicians, whereas Aristotle was the son of a doctor, but of course, he was thereby not ignorant of mathematics. The practical counsel to be derived from Pythagoras, is to measure, and thus to express quality in terms of numerically determined quantity. But the biological sciences, then and to our own time, have been overwhelmingly classificatory. Accordingly, Aristotle by his logic throws the emphasis on classification. The popularity of Aristotelian Logic retarded the advance of physical science throughout the Middle Ages. If only the schoolmen had measured instead of classifying, how much they might have learned! Classification is a half-way house between the immediate concreteness of the individual thing and the complete abstraction of mathematical notions. The species take account of the specific character, and the genera of the generic character. But in the procedure of relating mathematical notions to the facts of nature, by counting, by measurement, and by geometrical relations, and by types of order, the rational contemplation is lifted from the incomplete abstractions involved in definite species and genera, with a complete abstraction of mathematics. Classification is necessary. But unless you can progress from classification to mathematics, your reasoning will not take you very far."
In modern times however, beginning with William James, the question of the existence of an essence for classification which is the basis for Aristotle's schema began to be brought into doubt. For example, a tree could be classified by a botanist as an organism, by the landscape architect as an aesthetic entity, by the theologian as a divine benevolence, and by lumbermen as a potential source of income, and all of these are equally good bases for describing and classifying trees and the relative merits of different trees in accordance with the way they satisfy the variety of criteria. Thus, books could be classified either by their weight and size from the point of view of the shipper, by color and geometric proportion from the point of view of the aesthete, or by content from the point of view of the student. It is true that in the case of books, perhaps the content classification would satisfy a greater number of people, but that does not remove the fact that they can be classified from a variety of angles. By the same token, the red twig dogwood tree is a beautiful object for the landscape architect but merely a weed for the forester or lumberman.

*Compare (Shera, J. H., 1951) and (Dreger, R. M., 1968).
When it comes to human behavior, what basis may we adopt for classifying it? There are surely as many criteria for classification of human behavior as there are for the objects mentioned above. We must recognize once and for all that there is no single criterion for classification.

Even chemical structure of objects, which seems to be so fundamental an essence, is not satisfactory when it comes to classifying taste of substances, 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, and we still do not know how to develop better tranquilizers from a mere knowledge of the structure of the substance that we are preparing. What then is left for us to do when it comes to classifying human behavior?

In search for a way out I turned to George Gaylord Simpson's discussion of the science of "Systematics." (Simpson, G. G., 1961). 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 leading to observed diversity.

It became clear that 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. We might liken the search for etiology in the classification of human behavior 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 now is develop ideal aetiologies through the use of scientific models which 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 social-cultural or ecological niche which 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 junctures 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 resultant of the particular kinds and schedules of reinforcement which he has been subjected to, (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 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 aetiology of mental disorders. (Zabin, J., 1969). Is it applicable to suicide?

There is one difficulty in all of this classification, and that arises 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; nevertheless, controlled speculation may show the way for the type of research required for producing a classificatory system of suicide that would be of value.

II. Why is it so difficult to deal with suicide?

First of all, 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 which may have been arrived at by a variety of paths. Unravelling the causes after the fact is wellnigh impossible.

Secondly, 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 can not be regarded as representative of the latter. However, there is some overlap between the unsuccessful and the successful, since some of the 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% of those who eventually succeed, and that is indeed a small number out of the total that need to be followed up.

Thirdly, one of the primary considerations in suicide is intentionality. The act itself is a mere accident, if there was no intention behind it, and
intention alone is not sufficient if the act is unsuccessful and in such
cases doubt may even be cast on the intention itself. Now, intentionality,
a preoccupation of philosophical psychologists like Franz Brentano, (Sullivan, J. J., 1966
and in its more popularly known form as volition, the preoccupation of
Ach, N., 1935
Moriziss Ach, ( ), have never recovered from the body blows which
behaviorism gave them, despite the pragmatic approaches of Brentano and the
experimental approaches of Ach. Bostrom in Bumke's Handbuch des
Bostrom, A., 1928
Geisteskrankheiten ( ) does have a chapter on the role of will in psycho-
pathology, but it is limited to dissimulation of suicidal intentions as far
as suicide is concerned. 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, and 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 e.g., delinquent behavior, criminal
behavior, sexually deviant behavior etc. Then there follows a region of
disordered behavior in which the deviation is accompanied by self-reports
of distress i.e. there is deviation from both local behavioral norms as
well as from self-report norms e.g., neurotic depression, phobias,
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 successful act, a deviation from norms in self report of distress. Whether it is a disease or not 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, disorder or disease has varied throughout time with the Zeitgeist (Rosen, G. 1971). The ancient Hebrews have recorded only 5 suicides in the bible and these were all related to defeat in war and 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 4 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 most of these suicides did not seem to be due 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, until it became 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. However, cases of suicide by mentally disordered individuals were well known in antiquity. It is likely 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; and would have been classified in our terminology as disorders (deviations from social and personal report, norms). During the middle ages suicide was regarded as a sin with all the implications of such a view for the soul and property rights. Finally with the coming of the age of enlightenment the focus moved away from sin and its personal character of a 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 19th 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 suicide arising 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 non-operative, 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 -- which will weight each of the component models and their interactions.

A. The ecological model

In examining the role of the ecological niche which a person occupies in relation to suicidal behavior, it becomes clear that this model has a longer history and more highly developed sophistication 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 (Durkheim, E., 1951) classic models, Lindemann's (Gordon, J. E., Lindemann, E. et al., 1950) stress on the elicitation of suicidal behaviors in the vulnerable hypererotic, Henry and Short's (Henry A. F. and Short J. F., 1954) 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, and Cassel's hypothesis regarding the role of poverty and

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 in harakiri. Egoistic suicide is the result of rejection by society or inability to integrate with the social milieu and anomie 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 congenitally blind whose defect is corrected. This intriguing hypothesis seems to require some experimental testing. (Lester, D., 1970).

Evidence for the tenability of Durkheim’s hypothesis is amply illustrated in the rise of suicide rates during economic depressions,
decline during wars, stability of natio-cultural rates as long as their ecological niches remain stable. One social scientist, Cassel, (Cassel, J., 1962) and (Cassel, J. and Tyroler, H. A., 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 and 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 troika of disorders be given a common name: schizotubercide!

Another hypothesis emanating from the ecological model is that due to Gordon, J. E., Lindemann, E. et al., 1950) Lindemann, (He stipulated that Hypereridism, a morbid state of hostile aggression, exists in some individuals which 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, G., 1971) that suicide was the consequence of an emotional disorder a 'quiet rage' which 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 rental rates in an area with suicide rates in the same areas, 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:

$$\Sigma XY (\text{total}) = \Sigma XY (\text{within}) + \Sigma XY (\text{between})$$

which can be readily transformed to the weighted sum of corresponding correlation coefficients multiplied by appropriate standard deviations (Zubin, J., 1941; unpublished).

Usually, sociologists and ecologists in general 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, J., 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 and are often ignorant of the fact that the correlation in areas other than those they are acquainted with may be quite different. 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 & Dunham, 1939). In subsequent investigations by Gerard and Houston (Gerard, D. L. and Houston, L. G., 1953) and Hare (Hare, E. H., 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, and 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 plays an important role in suicide rates also. It is interesting to note that according to Stengel, (1958) social isolation is one of the few dimensions which plays 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.

B. Developmental Model

The most popular developmental model for suicide is that due to Freud and I need only mention it here, since it is well known to you. 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 (Gunch & Barraclough, 1971). Hendin's hypotheses regarding the variety of child rearing practices which differentiate the Swedes and the Danes with high suicide rate 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" which 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 of those who finally succeed ever make use of the suicide prevention centers, and 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, J. B., 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 had been leading a very independent life with a definite career who have a high suicide rate and contrast them with the older women who had been leading
a relatively sheltered dependent life who also have a high suicide rate. It is possible that in both these groups, a shift in locus of control may have taken place which brought about the crisis leading to suicide.* 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 and thus produced the crisis leading to suicide, and vice versa for the older women.

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 which 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 constraints 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 the four-fold table.

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

Expected suicide and homicide rates according to degree of environmental constraint and locus of control

<table>
<thead>
<tr>
<th>Environmental Constraints</th>
<th>High</th>
<th>Low</th>
</tr>
</thead>
<tbody>
<tr>
<td>Locus of Control</td>
<td>High homicide</td>
<td>High suicide</td>
</tr>
<tr>
<td>Internal</td>
<td>rates</td>
<td>rate</td>
</tr>
<tr>
<td>External</td>
<td>passivity</td>
<td>escapism</td>
</tr>
</tbody>
</table>

Following Henry and Short's analysis we would expect conflict to arise when an internal locus is paired with high environmental constraint producing a high homicide rate. When an internal locus is paired with low environmental constraint, high suicide rate should follow, since when something goes wrong, he can not blame 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, but when an external locus is paired with low environmental constraint a vacuum is produced, since there are no external guide lines for the external locus of control to follow. Confusion, aimlessness and goalless activity may ensue which may open the gates to escapism into drugs or hippy life. At all events, 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.
C. 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 give rise to the Zeigarnik effect and to the compulsive necessity for completion. Ach (\textit{Analyse des Willens}, 1935) in his monumental book, 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 video tape 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.

In contrast, to return to the notion of modeling, we might wish instead to present such an individual with demonstrations of alternative (non-suicidal) 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 non-suicidal 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.
An interesting angle on interrupted activity as a source of suicide is provided by Bryan's study of high class call girls. (Bryan, J. H., 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 entering on her calling. While 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 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 which elicit suicidal behavior (including ideation) and which ones 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, or accidents, unduly frequent exposure to hazardous situations, etc., could lead to a variety of reinforcements, which, especially if intermittent, could make the tendency to engage in such behavior very strong. The additional massive reinforcement by way of the attention likely to follow an unsuccessful
suicide attempt might then all but guarantee a repetition of the behavior, unless intervention, through reinforcement of incompatible behavior, were undertaken. It should also be mentioned that Miller's (Miller, N. E., 1944) positive-negative goal-gradients might provide another framework in which to examine this behavior.

Apparently, at least in our culture, learning to adopt the sick role during time of great internal stress, such as a severe depression, leads to beneficial results according to some authorities. Most people tend to learn to adopt the role. Those who do not, find it more difficult to adapt to the stress and suicide may result. Hence, the capacity of an individual to adapt himself to the local mores with regard to the sick role may be an important dimension to look into (Motto, 1965). The fact that only 4% of suicide attempts and even a smaller percent of the eventually successful suicides called suicide prevention centers (Kiev, 1969) may be taken as an indication of their inability to assume the patient role. A further indication of the importance of the assumption of the sick role is the high rate of suicide in physicians, especially psychiatrists who according to some are notoriously incapable of assuming the sick role.

Since the inability to assume a patient role and request help seems to be characteristic of some suicide-prone individuals it might be necessary to introduce educational methods for inculcating the need for seeking help when such help is warranted. The tendency to seek help when needed seems to run contrary to some of the spartan attitudes prevalent in the U.S. The history books and novels tend to glamorize the self-made man, the one who is equal to all occasions and who rarely needs help or seeks it. A more realistic
approach in which seeking help when needed is glamorized may reduce the uneven balance in our literature for the young. Family therapy with parents who inhibit dependent or help-seeking behavior in their offspring also seems a desirable step.

One of the primary factors in suicidal behavior is intentionality and as we had noted before, measuring such a subjective phenomena presents many difficulties because it has to be inferred. Yet it is the factor which differentiates homicide from murder and accident from suicide. It would be 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 attitude. The first steps in this direction have indeed been taken by Dr. Beck in this volume. He documents retrospectively all the events prior to suicide which 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 which may be relatively freer of the above mentioned difficulties. 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. (e.g., Salzinger & 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 and the sudden drop off of benign statements regarding the future when reinforcement ceases might be used as an indicator of high intentionality.
D. 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 has been reported. 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.

E. The Internal Environment Model

Bunney, W. E. Jr., et al. 1969

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. 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. The capacity of reserpine to elicit suicidal behavior is another problem calling for investigation.

F. Neurophysiological Model

There is no direct data on neurophysiological aspects of suicidal behavior, but there is one aspect of suicidal behavior which can be approached experimentally through neurophysiological considerations. In the processing of information that goes on in the brain of the would-be suicide, there seems to be a tendency for him to be fixed on a course of self-destruction as if no other options were available. In fact, it seems as if he is reversing the
steps in the search for identity which characterizes 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 other options were eliminated and how they could possibly be reopened is indeed an interesting problem. An indication of how a loss of freedom of choice affects behavior is offered in the following study.

A recent experiment (Glass, D. C., 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 fare much better subsequently, 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 levelling 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 non-suicidally 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 operate 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 et al 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 model seems to contribute least, with the internal environment and learning models somewhere in between.

The above mentioned scientific models were focussed on etiology. Of course, one could focus on treatment models, on actuarial models for prediction, 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 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 convulsive 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 the 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 with successful suicides. The right end of the distribution may contain two groups, one which is simply an extension of the normal distribution of the extremely suicide-prone and another group which is catapulted into suicidal behavior either through environmental stresses, psychopathology or even perhaps genetic predisposition.

A similar bump in the end of the distribution of intelligence was found by Roberts, J. A. F., 1950

Roberts ( ) which also was 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 (Zubin & Fleiss, 1969) for finding the clusters of individuals who are like-minded may help in separating extreme group of the successful suicides from those who do not belong with them.

Another problem that we are faced with is the question of prognosis versus prevention. In accordance with our earlier discussion, individuals who can not 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 the other contributors in terms of ecological variables and age and sex are the markers which are needed to locate the suicide prone. Elsewhere (Zubin, J., 1970) I have pointed out that as far as mental disorders are concerned, they 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 can not cope and give no evidence of
improvement. They are often regarded as still suffering with the disorder
even though it had long disappeared as is the case with the social break-

Thus, the new function for therapy is not to "cure" the disorder, but
instead to remould 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 their locus of control and its stabilization are
essential if future attempts are to be prevented. (Zubin, J., 1970)

Actuarial versus clinical prediction is another problem that has been
raised. In an earlier discussion (Zubin, J., 1955)
I have pointed out that this is in reality 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 which 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, however, 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 and once the data are collected, clinicians the highly skilled, 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 which 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, J., 1969).

Another issue that often arises deals with the development of measures of agreement between two raters. Often, the two raters are matched in a four fold table and the value of $X^2$ for the 4-fold table is computed as a measure of agreement. It must be recognized, however, that for the purposes of a measure of agreement Kappa is much superior (Fleiss, J.L., Cohen, J., & Everitt, B. S., 1969) since $X^2$ is a measure of association which is fed equally well by deviations from chance disagreements as well as deviations from chance agreement. Kappa measures only deviations from chance agreement.

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

Regarding the possibility that all suicides are mentally ill, it is of some interest to note that according to Robbins, (Robbins, E., et al, 1959) fully 94% of successful suicides in St. Louis were found to have been psychiatrically ill when detailed clinical information was obtained from relatives, only 4% were terminal medical cases and only 2% appeared to be well.

Similar findings have been reported by Barraclough in this volume on a study conducted in England. It is interesting to remember that during
the 17th and 18th century the notion was widespread that England was
the land of melancholia and suicide and 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
Barraclough'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.

One is reminded that 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, such as might be afforded by accident mortality
cases, e.g. automobile fatalities. Though this source might not be entirely
free of the suspicion of suicide, it would nevertheless provide a check on
the question whether the search for evidence for mental disorder which was
undertaken in the post-suicide 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 in individuals who are incapable of benefitting from it, only
time can tell. There is, however, a choice bit from Eugen Bleuler (Bleuler,
E., 1950)
which I would like to call to your attention exemplifying this problem.

"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 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, into 'as if' causes known 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 which characterize the niche which the
high risk group occupies.

For the developmental model, we need an assessment of their life history
and how they underwent the critical stages of development. Here only inter-
viewing 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 their
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 which may be useful for testing the neurophysiological model.

What I am suggesting in short, 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 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% of the group we are to follow-up will yield pay dirt in terms of our expectation, but a successful study of these 15% ought to prove very rewarding in prognosis as well as in prevention.

If we could combine Dr. Pokorny's descriptive approach with the etiological models described here, research in suicide behavior might become more fruitful.
References


Glass, D. C., Reine, E. and Singer, J. E. Behavioral consequences to adaptation to controllable and uncontrollable noise. *Journal of Experimental Social Psychology*, 1971, 7, 244-257.


Hare, E. H. Family setting and the urban distribution of schizophrenia. *Journal of Mental Science*, 1956, 102, 753 ff.


Motto, J. A. Suicide attempts. *Archives General Psychiatry*, 1965, 13, 516-520.


Zubin, J. Note on Dr. Thorndike's "The fallacy of imputing the correlation found for groups to the individuals of smaller groups composing them. In American Journal Psychology, 1939, 52, 122-125. (Unpublished manuscript, 1941).