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## IS PAROLE PREDICTION A SCIENCE?

RAY L. HUFF<sup>1</sup>

When we find ourselves unable to agree with our friends, it seems to be appropriate that we confess and point out the path our thinking follows. Perhaps we can thereby be led to a point of mutual understanding from which more exact thinking may proceed. With this premise, I confess a lack of agreement with the recent article in the *Journal of Criminal Law and Criminology* written by William F. Lanne, which he titled "Parole Prediction as Science." XXVI: 377. I use the statement of the "Character Education Institute, Washington, D. C.," to define the scientific method through which the constituent parts of a science are developed. Taking these steps seriatim, we should examine the studies in the field of parole prediction to determine their sufficiency as revealed by this measure.

1. "Gather data on the problem within a selected field according to some adequate, sound plan by means of numerous and accurate observations made with the human senses assisted and corrected by instruments of precision. Observations must be recorded in definite terms and measurements and in specific statements."

In the studies presented, data have been gathered in keeping with a plan to relate a series of specific categories to success or failure of parole. Most of the students defined successful parole as the termination of parole without the issuance of a warrant. Glueck projected this period to a time after the sentence has ended.

If a science were established, there would be no differences among competent students about the basic measure. There would be unanimous agreement that the time interval was the sentence or that it was not. Students would agree as to what is being measured.

It seems to be clear that if we are measuring parole our time interval should be that in which parole control exists. Are we measuring the immediate effect of parole control, or are we measuring a degree of persistency of the achievements of parole projected into the future? Shall we say one year after parole control

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ends, or ten years, or must we wait until the subject dies before we can measure the effectiveness of parole procedures? Shall we name what we do in work in parole "procedures," or shall we think of parole as a succession of discrete acts? What we decide will limit the field in which our data will be gathered. Is Glueck's idea the one basic for the time interval, or is some other time the proper unit? How can we secure comparable results if we do not have a common measure of success?

When parole control ends, we can draw a line and say this parolee completed his sentence and a warrant did not issue, therefore, it was a successful termination. But even this modest measure presumes that a warrant will issue for the first violation. Is the violation a failure to report, leaving limits, failure to observe a special condition, or some other violation of a general or special rule of peculiar value in a particular case? Or, shall the violation be evidenced by a record of an arrest, while under parole control? If so, shall the arrest suffice or must there be a conviction? If we say there must be a conviction, shall it be a conviction for a traffic violation, for getting drunk, for a misdemeanor or felony? If we had a science, this point should be clear for the purpose of measurement. Or, is it to be argued that the judgment of the paroling authorities is the judgment of those "qualified to judge" and, therefore, a sufficient measure? Or, on the other hand, shall we measure degrees of improvement in social status to secure measures of success?

If we raise only these points in an effort to limit our field of inquiry, is it not clear that the measure of success (as indicated by time under supervision) is limited originally by action of the court, and not necessarily by the parole authorities, where there is a definite sentence? Where there is an indefinite sentence, are the parole authorities able to control the time to terminate control, i. e., end of the sentence? If they are, is it generally understood at what time that termination will occur, or, would we be correct in understanding that the parole authorities could bias the results by premature terminations?

The fact is that in parole jurisdictions, generally, the nature of the sentence is not the same, the method of preparation for parole, the practices of parole, the very principles on which parole operates differ in fact and degree. The plan in accordance with which data are collected, and the field which is defined from which data are collected, are not clearly marked. The field of inquiry is not defined beyond dispute.

Nevertheless, the students reporting offer data which has many similarities. The fact that it has these similarities is impressive even though the questions stated above remain unanswered to our satisfaction. We hold reservations because of this fact, but, tempted by the desire to believe, let us examine the data to determine whether they can satisfy the demand for "numerous and accurate observations made with the human senses, assisted and corrected by instruments of precision."

The data, as presented, are categories of personal and social facts: age, marital status, type of neighborhood, intelligence quotient, etc. Let us agree that instruments of precision have been used in determining mental age, physical condition and other results of examinations given by physician and psychologist. We agree to this only tentatively. Are the criminal record, social history, school history, psychiatric report, institutional conduct record and other similar categories precisely correct data or are these data secured in unrefined state? In other words, are the records reliable from which the data have been gathered for use in the study defining the "science"?

Is it a sufficient test of reliability to have the records examined by more than one examiner or by one examiner more than once? If the answer is "yes" then perhaps we should question the validity as distinguished from the reliability of these records. Our question is then—Do the records of the prisoners tell the essential facts about them as subjects for parole? Are the prisoner's records developed from the prisoner's statements, from those of his friends, or enemies, and are they collected (and collated) by persons competent to judge validity? Are the perception, judgment, and skill in recording of those who compiled the data sufficient to make this question unreasonable?

Glueck's study and the day by day experiences of parole officials throw serious doubt on the validity of prisoner's records. Are they valid in half the cases, and not in more than half? If the basic data are not valid, how valuable is any finding which follows careful work with them? If such categories of age, nativity et al, are valid and other categories of marital status, previous record, et al, are not valid will the inclusion of invalid data with valid data permit a statement of fact which will meet the demands of science for precision? Even though the terms which define the categories are definite, is an invalid perception and judgment made valid by such definiteness of name? If, for example, marital status is to be recorded "married, widowed, single, divorced, separated," do we have

a valid record of the "married" status when the prisoner was living with a mistress, while his wife supported herself by prostitution? Or is an "X" after "married" correct, when the legal wife is a termagant? Would the use of a large number of cases permit examples like these to be absorbed into a norm? Is it presumed that the corrective processes which are used to eliminate such examples are as applicable to the prison population as to the general population? Or, can we agree that the laws of normal chance do not operate with this prison group? Are they actually a group selected without bias in the scientific sense?

More questions could be raised about the data which have been gathered. Before we can agree that parole prediction is a science on the basis of the data offered to date, we need to be satisfied that the categories are representative of accurate observations made with "human senses skilled in perception" and able to refine this perception through experience, or otherwise correct it to eliminate "misunderstandings, superficiality and bias."

At this point, we should anticipate further difficulties in agreement. We observe that the data appear to be limited to facts about the prisoner. Where they include the community, they date to the past. Is it to be presumed that nothing will be done with community conditions or the prisoner's condition from the time these categories are recorded? Can the community be considered static and not dynamic?

We could not agree to a premise that the factors affecting parole are static, or that they are exclusively, or even primarily, concerned with the prisoners? The data must include measures of *change* in prisoner and community relationships, if that which is to be measured is parole in its modern conception.

2. "Classify and organize data on the basis of similarities, variations, activities, processes, causes, and results."

If we are able to feel sure about the original data, we could feel safer in recognizing the fact that the data have been classified and organized. However, we observe that classification proceeds from a conception of a measure of success and failure on which there is disagreement, and which is affected by the nature of the sentence imposed. We think, too, it is affected by the kind and effectiveness of the parole procedure itself. Since such procedures vary, we will have difficulty in understanding that there is finality to be secured through the careful classification which has been given the data.

We question not only the validity, but also we question the suffi-

ciency of the classification used. Are the norms suitable in terms of a group which is unrepresentative of the general population at the time of classification, and which is a member of the general population during the time of testing? Does a similarity by name of category imply the same meaning to different students? Can we neglect the personality and skill of the supervisor?

3. "Generalize to get principles and theories into tentative form."

As we understand it, the generalization agreed upon is that the experience of one year or with one group of prisoners will be repeated. This does not seem reasonable unless we can believe that the parole problems and processes are static. This is not a fact as we see it. If it is a fact, the parole procedures of the system in which the study is made are already obsolete. It should be inconceivable that a known unfavorable fact would be allowed to remain uncorrected or at least unmodified. The degree of chance should be changed currently.

4. "Verify generalization by controlled experiments, by tested predictions of results and by repetition of experiments."

Have the generalizations been verified by controlled experiments? Are we to understand that the statistical treatment to distinguish significant differences is sufficient, or, are we justified in asking what has been established through the method of tested predictions of results?

As we see the data, a number of series of categories have been tested and certain ones have been found to be more frequently associated with different measures of success on parole than can be attributed to chance. Given the same categories, will the experience be repeated if a new group of cases are tested?

We think the premise that it will be repeated is not sufficiently established by the data offered.

Seeking to profit by the work which has been done, 5912 records of Federal prisoners were examined, and an adaptation of the Burgess-Vold method was applied by the writer and the staff of the Federal Bureau of Prisons. Results were secured which were in agreement with the findings on which the article "Parole Prediction As a Science" was based.

However, on the premise to be tested, the "prediction" scale developed from the 5912 cases was applied to 3000 cases, by Barkev Sanders, Research Analyst of the F. E. R. A., and the scale did not "predict." While there was a remote tendency to conform, given categories hitherto found to be significant in one direction were

found to be significant in another: this in spite of the fact that practically the same success rate for the total population persisted and that all *procedures* of preparation, selection, and supervision remained practically constant.

It is pertinent to state here that in 1932, using a random sample, case control technique, a group of violators and non-violators were examined to discover characteristic differences between them. Among others, it was found that records showing a plus Wasserman at admission showed a violation rate sixteen times as high as was shown when no plus Wasserman was recorded. This Wasserman group violation rate was four times the rate for the total (unselected) group. The succeeding 500 cases showing plus Wasserman were marked for examination. Preparation, selection, and supervision procedures were adjusted to meet the issue. The violation rate for these 500 cases was one-fourth that of the total parole population as measured by the same time interval. This reversal of tendency was produced, solely, as far as is known, by an adjustment in procedure. Had there been no adjustment, would the first experience have been repeated? Does the fact that an inquiry was made, and a fact established, justify us in stating—"Parole Prediction Is a Science?" Is the effectiveness of a plus Wasserman universal? Is it a physical, mental, or social factor?

Are we able yet to use a measure in parole as is used in the change of condition in a narcotic addict? When declared an addict, the subject's blood chemistry is condition "A". After treatment it is condition "X". When it is condition "X" the addict is no longer an addict. Will he revert to the use of drugs? This is a parole question. It is not certain he will not. Many do. Does this one chemical fact create a science of parole?

Have the students to date done more than discover discrete facts? Do such facts lend themselves to the test of repetitions? Does a twenty-one year old face the same prospects as a sixty year old persons with all other factors constant?

Before we can believe parole prediction is a science, we will need to see existing studies applied to new and controlled situations. Adequate data must be much more inclusive than at present. Categories, including public policy, which may deny parole in spite of all the present categories being favorable will have to be included. All the categories which are or need to be included in the dynamic processes of changing, not the prisoner alone but the family situation at least, must be included. The categories will need to be so arranged that the numerous elements are measured which enter

into the evaluation of the milieu in terms of changing factors which is now done by the exercise of judgment by parole officials. This is an art. Perhaps that art can be resolved in part into the variable elements which compose it. Efforts to do so are praiseworthy, but, to date, are not convincing.

Perhaps it will be some time before we can name all the categories necessary to give a satisfactory body of facts about parole. Is it correct to assume that only that which has been measured is true? Or, can we assume that with improvement in methods of measurement many more items of truth can be stated objectively than is now practical?

There has been much that is new in the application of mathematics through statistical methods. As we see parole, there is much more that mathematics needs to do (and parole officials too) before it will be competent to measure the complex dynamic social processes necessary to be measured if parole is to be named a science in mathematical terms.

It is possible to find objections against the use of the "scientific method" as recited in this paper. Perhaps there are other tests which should be applied. Regardless of what measure may be used, it is certain that the validity of the data studied needs to be established. It is at this point that studies in the criminal field are weak. Moreover, the premise needs to be in accord with principle. Any research in the parole field should be premised on the fact, that in any effective parole system, changes in social situations should be the rule and not the exception. Facts as we find them are amenable to change, and in any effective system will be changed in some degree so that the experiences of one period need not be the experiences of the succeeding period. It is the social situation in which the prisoner is an element which should be the basis of measurement as distinguished from the prisoner alone.

We have too many facts which meet us daily in parole work, which are not accounted for by the hypotheses presented by the reported studies, to be persuaded the time is here to call parole a science.

Experimentation should go on. Communities and processes need to be brought into the measures as well as their interrelations with prisoners, before the experimentation is well established.

Even so, the present studies are helpful to clarify thinking, to challenge ingenuity. If they are recognized to be tentative findings of fact, if we are cautious about premature conclusions, and beware of *non sequitur*, they will continue to be helpful.

Or in spite of these doubts, is parole prediction now a science?