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Scientific Status of Parole Prediction

Ferris F. Laune

THE SCIENTIFIC STATUS OF PAROLE PREDICTION

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FERRIS F. LAUNE*

In an article entitled "Is Parole Prediction a Science?", which appears in the present issue of the JOURNAL, Mr. Ray L. Huff points out the desirability of an open interchange of views between investigators in the same field, as a means of clarification of issues and mutual rapprochement of opinion. The present instance is a splendid case in point. For it is our belief that Mr. Huff's views concerning the scientific status of parole prediction and our own are very nearly identical; the only points of difference seem to rest upon misapprehensions arising from lack of clarity in our previous statement of our point of view. The present brief note is an attempt to correct that lack of clarity.

In an article entitled "Parole Prediction as Science," which appeared some months ago in this JOURNAL,[1] the point of view was expressed that parole prediction as hitherto practiced constituted a fine art—a series of more or less completely independent attempts by various investigators to work out a methodology which would make it possible, to some degree, to foretell the probability that a given inmate of a penal institution would, when released upon parole, "succeed" in the sense that he would not be declared a violator of his parole. The opinion was advanced, however, that there now existed a sufficient corpus of experimental findings in this field to permit of a critical analysis of the various methodological techniques—to isolate those elements of the techniques which, by their consistency and universality, seem, in contradistinction to the purely fortuitous factors, which vary from study to study, to be, in some manner, inherently connected with the quality under investigation: probability of success on parole. Finally, it was advanced as our belief that the factors employed in prognostic tables must, if parole prediction is to become a science, be rigidly limited to such as can be shown to possess four fundamental criteria: reliability, significance, orthogonality and stability.

Our point of view, then, as expressed in the article mentioned, is in very close agreement with that of Mr. Huff. There do appear to be a few points, however, upon which a little further discussion might prove fruitful.

Mr. Huff, for example, questions the right of parole prediction to be considered a science, on the ground that there is no general agreement as to what constitutes success on parole. No universal chronological period has been adopted, after completion of which an individual may be considered a success, nor indeed, is there any general agreement even upon so fundamental a matter as just what events: return to institution, issuance of a warrant, etc., shall be considered to constitute a violation of parole. We are in entire agreement that this situation is deplorable, and, indeed, one of the points brought out in our previous article was a strong plea for the adoption by all investigators of a unified methodology.[2]

But we do differ from Mr. Huff in our view of the nature of the units universally used in the physical sciences. It is true that all physicists agree in using the meter as the unit of linear distance, and, further, that they all agree as to the exact length which is to be called a meter. But this particular length is in no sense a unit existing independently and given the scientist ready-made by external nature. Far from it—a meter is, by definition, the length of a certain metal bar kept under glass in Paris, or, of course, of any other bar of exactly equal length. And the uniformity of physicists in accepting this definition of a meter is the outcome of a perfectly definite, conscious agreement on their part, and is in no sense an external fact in nature. There is nothing sacrosanct about this particular unit of length—any other might have been selected. The important thing is that *some* unit be universally agreed on. We agree with Mr. Huff that it would be highly desirable—indeed, that it is almost essential, that investigators in the field of parole prediction, also, agree in adopting some universal unit of time, and some universal, well-defined criterion of what constitutes a violation of parole.

We say "almost essential" because it does seem possible to attempt the synthesis of various studies even in advance of the adoption of universal units. While it is more convenient to the physicist, accustomed to dealing with the metric system, to have his data expressed in centimeters, grams and seconds, he can, nevertheless, make his calculations also with data expressed, instead, in inches

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[2] *Op. cit.*, p. 392.

and ounces. All he need do is perform the simple mathematical operations indicated by the formulas which convert inches to centimeters and ounces to grams. Where, in parole prediction, varying parole periods or varying criteria of violation are employed, the matter is not quite so simple. It is, nevertheless, quite feasible to convert the data of one such study into terms comparable with those of another by employment of partial regression equations.

Let us assume, for example, that we desire to measure the effect of length of incarceration upon probability of success on parole in each of several studies, which, however, fail to agree in the period of parole supervision considered. We believe that this difference in period on parole may have an influence upon the weight of the factor considered: that a long term of incarceration may have less influence upon probability of success when only a year after release is considered than in those cases where the parole period is three years. By the method of partial correlation, we find the partial correlation between length of incarceration and success on parole *when the length of parole period is held constant*. And when this partial correlation coefficient has been calculated, it is easily possible to express the results of any study in terms of a one-year parole period, or of a three-year parole period, or of any other unit we may choose.

The same technique may be applied also to varying criteria of parole violation. Here, however, it does seem highly advisable to adopt a hard and fast practical rule. The criterion used by Burgess, Vold, Warner, Tibbitts, Laune, and Van Vechten—the purely pragmatic criterion that a violation of parole exists where, and only where, a warrant issues, seems to be the only feasible one in situations where elaborate follow-up studies are financially impossible.

A second important question raised by Mr. Huff is that of the validity of criminal data generally. That it is obviously impossible to reach accurate, valid results from employment of inaccurate and invalid data, no matter how refined our mathematical treatment of the rough data, we agree. That criminal statistics vary widely with regard to their statistical reliability and, therefore, presumably also with regard to their objective validity has been incontrovertibly demonstrated by Sutherland and Van Vechten.[3] But that unreliability of data or invalidity of results of observation is confined to

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[3] Sutherland and Van Vechten: "The Reliability of Criminal Statistics," Journal of Criminal Law and Criminology, XXV:10.

parole prediction or to the social sciences generally, we cannot admit.

Professor René Blondlot of the University of Nancy was generally acknowledged to be a skilled and careful observer of physical phenomena. His reputation as a physicist was international. Yet, in 1903, he announced the discovery of the so-called N-rays. In the past thirty years no other physicist has succeeded in observing this phenomenon; the probabilities are that Professor Blondlot was deluded and that his experimental results are not valid. Surely, however, the existence of such cases does not constitute a sufficient reason for despairing of the scientific nature of physical findings generally.

Some of the studies in parole prediction contain very careful and elaborate field investigation as a check on the validity of record material. Wherever such independent verification is possible, it is of course highly desirable and increases the value of the data many-fold. But even where no independent verification of record material exists, we believe that there is justification for accepting the raw data in lieu of more accurate findings and that in the case of certain factors whose reliability is very high—such, for instance, as race and nativity—the validity of the data is sufficient to warrant their inclusion in prognostic tables. As regards those factors whose statistical reliability is low, we adhere to the opinion set forth in our original article:[4] that they should not be used for prediction. This would be true, of course, also in the case of factors whose validity could be demonstrated to be low.

Mr. Huff further objects that experience tables necessarily point to the past; prediction points to the future. The objection is valid; times do change. But, is it unreasonable to suppose that as our technique develops we shall be increasingly able to measure, at least partially, those community factors whose change affects violation rates and to allow for them in our total prediction? Further, do not general, wide-spread community factors, such, for instance, as the economic depression, tend to affect all parolees more or less uniformly, so that, while it may perhaps raise the general violation rate of all parolees, it tends to leave the relative probability of success of the various individual parolees unaltered?

The statement that so highly complex a phenomenon as behavior on parole depends much more largely upon dynamic than upon static factors, is entirely in accord with our own opinion. To

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[4] Lanne, Wm. F.: *Op. cit.*, pp. 385-386 and p. 400.

meet this difficulty we have attempted to develop a technique for measuring attitudes of delinquents and to predict parole success on this basis. A description of this technique is contained in Laune's doctoral dissertation[5] and also in a book, soon to appear under the title "Predicting Criminality."

Mr. Huff inquires whether parole predictions have ever been employed in controlled laboratory experiments. In the social sciences generally, the employment of laboratory control presents unique difficulties. Such attempts have, however, been made, notably by Vold.[6] Our own laboratory is at present engaged in testing predictions by controlled laboratory methods. The results, however, will, in the nature of the case, not be available for several years.

In this connection it might be stated that the criterion of stability, originally recommended in our previous article,[7] is intended to supply a part of the function of experimental verification. It seems reasonable that if a number of investigators, working under widely differing conditions and with widely differing populations, are in accord in certain of their results, those results may legitimately be said to have stood the test of repeated experimental trial. Indeed, with the concept of stability firmly in mind, it even seems possible to compose a prognostic table of relatively great accuracy for use in a situation *where no experience table exists,* i. e., in an entirely new situation. For, if we limit ourselves strictly to those factors which are universally stable in all the studies, it is extremely probable that they will be found to obtain, in the same sense, in the new situation also.

Finally, Mr. Huff asks whether only that which has been measured may be regarded as true. Facts which have never been measured nor quantitatively expressed may no doubt be true, but we believe with Francis Galton that "Until the phenomena of any branch of knowledge have been submitted to measurement and number, it cannot assume the status and dignity of a Science."

5 Laune, Ferris F.: "A Technique for Developing Criteria of Parolability," doctoral dissertation filed at the Northwestern University Library.
6 Vold, George B.: "Prediction Methods and Parole," Sociological Press.
7 Lanne, Wm. F.: *Op. cit.,* pp. 388-395.